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NOTICES OF The Aëronautical Society.

At a meeting of the Council of the Aëronautical Society of Great Britain held at the Society of Arts, John Street, Adelphi, on October 19th, 1903, the following were elected Members of the Society:—

MAJOR-GENERAL SIR HENRY E. COLVILE, K.C.M.G., C.B.
Miss EVE FAIRFAX.
MR. FREDERICK SHAW KENNEDY, J.P.
MR. GEORGE CLOUT.
MR. THOMAS MOY.

At a meeting of the Council held at the Society of Arts on December 3rd, 1903, the following were elected Members of the Society:—

DR. HUGH ROBERT MILL, D.SC., LL.D.
Secretary Royal Meteorological Society.
MR. R. RANDEL PHILLIPS.
MR. LAWRENCE HUGH STRAIN.
MR. J. J. SNOW.

Dr. Hugh Robert Mill was elected a Member of the Council.

The next General Meeting will be held at the Society of Arts, John Street, Adelphi, in March, the date of which will be duly announced. Amongst the papers will be one on "The Measurement of Air Velocity and Pressure in Aëro-Dynamic Experiments" and "The Balloon Anemometer" by Professor A. F. Zahm, of Notre Dame University, Indiana, U.S.A., and "Mechanical Flight" by Mr. Thomas Moy.

THE LIBRARY.

Arrangements have been made for the Library of the Society to be open to Members for reference and the borrowing of books on the first Monday in the month from 2 to 5 p.m. commencing Monday, March 7th.

The following books and publications have been kindly presented to the Library:—

By the President, Major B. Baden-Powell: "Animal Locomotion," by Pettigrew; "Animal Mechanism," by Marcy; "Aëronautica," by Monk Mason; "Aëropaidia," by Baldwin; "The History and Practice of Aërostation," by Tiberias Cavallo; "Smithsonian Reports," various

By Colonel F. C. Trollope: "Reports International Aëronautical Commission, 1901 and 1903." By Royal Meteorological Society: "Exploration of the Air by Means

of Kites," by Lawrence Rotch; and "Reports of Glaisher's Balloon Ascents."

By Dr. W. N. Shaw: "Meteorological Observations obtained by the Use of Kites off the West Coast of Scotland, 1902," by W. N. Shaw, Sc. D., F.R.S., and W. B. Dines.

Members of the Aëronautical Society of Great Britain, and others who may wish to take part in the Aëronautical Competitions at the St. Louis Exhibition, are requested to communicate with the Honorary Secretary of the Aëronautical Society of Great Britain.

ERIC STUART BRUCE,

Honorary Secretary.

GENERAL MEETING.

The opening meeting of the Thirty-ninth Session of the Aëronautical Society of Great Britain was held at the Society of Arts, John Street, Adelphi, on Thursday, December 3rd, 1903, Dr. William Napier Shaw, F.R.S., in the chair.

The CHAIRMAN: I will ask the Hon. Secretary to read the Minutes of the last two Meetings.

The HON. SECRETARY read the Minutes.

The CHAIRMAN: The first business of the evening deals with the report of the International Kite Competition. The 25th June was a very remarkable day. In the first place, it was a fine day, and in the second place it was the first occasion so far as I know on which a kite competition has been held in this country. I must say that when the Aëronautical Society asked me to take part in the Jury on the Kite Competition, I did not realise entirely what was meant by the undertaking, and I was surprised, first of all at the difficulty in arriving at a satisfactory way of estimating the performances of the kite, and then surprised at the way in which these difficulties were overcome. That successful result was due first to Professor Boys who devised the means for estimating the altitude of the kites, and secondly to the Honorary Secretary, who undertook enormous labour, and with very great success, in getting over all the practical difficulties, and converting what must have appeared to a number of people in Worthing as a picnic into a veritable kite competition with careful measurements of the heights of the kites, and everything else arranged as it should be. I will first of all call upon Professor Boys to give us an account of the method of determining the altitude of a kite.

The Aeronautical Society's Kite Competition.

I.—Mathematical Portion.

By PROFESSOR C. V. BOYS, F.R.S.

When in the spring of this year your persuasive and energetic Secretary asked me to act with certain other scientific gentlemen upon a jury to decide as to which of a number of kites should attain and maintain, for a time, the greatest altitude, he did not have to exercise his special attributes long before I was happy to undertake to do what I could to make the competition a success.

Of the admirable arrangements made under his auspices it is unnecessary for me to say anything; they are well known to you all. The first question which the Jury had to decide was, how should the actual altitudes be determined? Whatever system might be decided upon, difficulties, or an actual breakdown, were by no means unlikely.

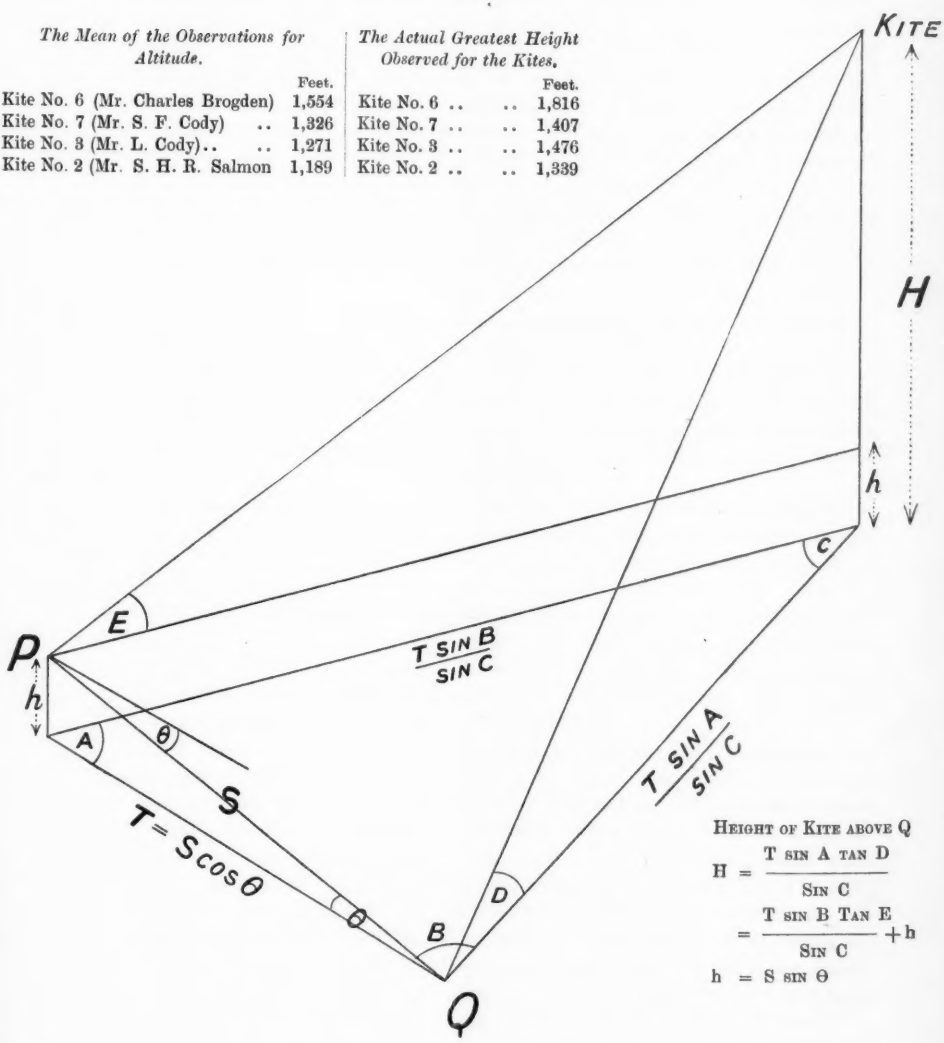
The kites, if they attained a goodly altitude, might well be lost to sight among clouds, and any optical method would surely fail. The determination of the height by measuring the angle of inclination of the wire, and the length paid out, and by making an allowance, not easily determined, for the sag would not suffer

RESULTS OF INTERNATIONAL KITE COMPETITIONS.

Diagram for Theodolite Observations of Kite Competitions.

The Mean of the Observations for Altitude.	Feet.
Kite No. 6 (Mr. Charles Brogden)	1,554
Kite No. 7 (Mr. S. F. Cody) ..	1,326
Kite No. 3 (Mr. L. Cody).. ..	1,271
Kite No. 2 (Mr. S. H. R. Salmon	1,189

The Actual Greatest Height Observed for the Kites.	Feet.
Kite No. 6	1,816
Kite No. 7	1,407
Kite No. 3	1,476
Kite No. 2	1,339



HEIGHT OF KITE ABOVE Q

$$H = \frac{T \sin A \tan D}{\text{Sin } C}$$

$$= \frac{T \sin B \text{ Tan } E}{\text{Sin } C} + h$$

$$h = S \sin \Theta$$

To face page 2

from the difficulty of invisibility, but the process would be a slow one. Each kite station would require an observer, and means would have to be provided to measure the run of the wire. Moreover, when all was done, extreme accuracy would be out of the question, and it might be impossible to decide upon the relative merits of two kites of nearly equal altitude.

The uncertainty of the result would be greatly lessened if the angular altitude of the kite as seen from the kite station could be determined as well as the inclination of the wire, and the amount paid out; but then, if the method were to depend upon seeing the kite, why allow optical precision and quickness to be marred by the cost, trouble, and delay that would result from measuring the run and inclination of the wire?

Recording aneroids would remove all difficulty of measuring greatest altitude, if they could be trusted, and if the competitors, other than the winner, could be persuaded to have faith in them, but they would be useless for the purpose of ascertaining the behaviour of the kite over a period of time.

The jury in their considerations finally decided that it would be best to adopt an optical method and run the risk of not being able to see the kites. A small committee, consisting of the Chairman and myself, then undertook to elaborate such a method.

We were met by the difficulty that we did not know what class of observer we might have to depend upon, for we felt that theodolite observations, which were clearly those best suited to the purpose, should be made by someone familiar with their use in surveying, so as to be able to make quick and trustworthy settings, and to read the results without risk of error in the hurry, excitement, and general disturbance. We felt that we should not be called upon to make these observations ourselves, as there might well be enough to attend to without having this upon our shoulders as well. Thinking it likely that the Society might have to fall back upon the services of men who, though accustomed to handle and read the instrument, might not themselves possess much knowledge of trigonometry, we determined to prepare a diagram and forms, in which blank places would be left for the observations, and the observer should as far as possible be relieved of all responsibility, except that of implicitly and faithfully filling in the forms. In the same way the computor was provided with forms, where the process should again be as far as possible purely mechanical. Had we known the class of men that ultimately consented to perform these arduous duties, I do not think we should have thought it necessary to do anything to assist them. I may say at once that the ultimate success of the proceedings depended upon the rapidity and accuracy of Mr. Dallas and Mr. Mackenzie, of the Royal Engineering College, Coopers Hill, who made their observations in the most admirable manner. Similarly the results were computed by Mr. Mason, of King's College, upon the forms that had been prepared, but blank paper would have served his purpose just as well.

As some of the members of the Aeronautical Society may be interested in following the process of observation and reduction, a short description of the method adopted may be worth giving.

The theodolite is an instrument which, if it is properly adjusted, enables the observer to read the altitude of a distant object—that is, the angle of elevation above the true horizon. Referring to the perspective figure, a theodolite at Q will measure the angle D between the line joining the theodolite and the kite, and a truly level line passing through the theodolite and vertically below the kite.

Another theodolite at Q will in the same way measure the angle E between the line joining it and the kite and a truly level line vertically below.

Similarly either P or Q will measure the angle marked Θ in the diagram, which is the angle between the line joining P and Q and a truly level line in the same vertical plane.

P and Q are not likely to be at exactly the same level, and on such ground as the Findon Downs they are sure not to be. Actually, there was a difference of 35 feet, that is, h was 35 feet (but as on the ground Q was the highest this appears as -35 feet). Dropping now perpendiculars from the kite and from P upon the level lines drawn from Q directly under Q-kite and Q P, the level triangle A B C is formed, and is

thus defined. Now the theodolites at Q and P, besides measuring the two altitudes, each measure one of the two angles A and B, or the difference of azimuth of the kite and the other theodolite. It should be noted that these are not the angles kite-Q P and kite-P Q which would be measured with a sextant, but the horizontal angles obtained by projecting these on a map.

The two angles A and B of the triangle being known, the third angle C = 180°—(A + B).

The size of this triangle will be known when one side is known. Now, the line S joining P and Q is directly observed by chaining, and the side T of the level triangle being the projection of S is less than S in the proportion of I: Cos Θ, so the one side T = S cos Θ is known and is found once for all. The sides of a triangle are proportional to the sines of the opposite angles, and so are equal to T $\frac{\text{Sin B}}{\text{Sin C}}$ and T $\frac{\text{Sin A}}{\text{Sin C}}$ as shown on the diagram.

The height H of the kite above the level of Q = the line B C × tan D

$$= \frac{\text{T sin A tan D}}{\text{in C.}}$$

The height of the kite above the level of P = H − h. h is simply S sin Θ, and is found once for all. $H - h = \frac{\text{T sin B tan E}}{\text{Sin C.}}$

$$\text{or } H = \frac{\text{T sin B tan E}}{\text{Sin C}} + h$$

So there are two expressions for H, the height of the kite above the Q theodolite.

It will be seen that every complete kite observation depends upon the measurement of four angles, an altitude, and an azimuth by each theodolite. Three are sufficient, and with three only the height can be found, but with four a useful check is obtained, as if any one of the four is incorrect the height as deduced from the two expressions will be different. The result is that not only is the height measured but its correctness is proved; that is, it is proved unless at least one other error has been made of exactly such magnitude as to correspond with the first, a contingency which need not be considered.

What is wanted is not the height of the kite above either theodolite, but its height above its own winding drum or kite station. The several kite stations were therefore observed by the two theodolites both before and after the flights so as to determine their heights above (or below) the theodolite station Q. The algebraical difference between the kite result and the kite station result gives the height of the kite above its drum.

Such is the geometrical theory. To carry this out it is desirable to so choose the positions that the triangles do not become of extreme shapes, for in such cases, to use an analogy, optical leverage strains the observations to the utmost and such precision will not be obtained as with well-chosen conditions. If, for instance, the two theodolites were placed 10 feet apart all the observations could be equally well taken, but as the directions would be so nearly alike for the two theodolites a long thin triangle would result and the optical leverage would be such that an error of half a minute in the observation would throw out the result to a disproportionately large extent.

We therefore chose the two theodolite stations nearly half a mile apart in places where they could see each other and all the kite stations which spread out over about a mile. We hoped they would also be able to see the kites, and thanks to the weather we were not disappointed.

Another difficulty of the greatest importance, depending on the fact that the kites did not keep quiet but were apt at times to rise or fall very quickly, was met by the provision of a field telephone joining the two theodolite stations. Dr. Shaw and I established ourselves at the two ends of the telephone line and were able in this way to be in touch with the observer at each end, and so when both had the same kite under observation, and were ready, we could give the signal "now," and ensure simultaneous observation.

By previous arrangement those in charge of the kites started the flights on a signal being given with a flag. They had an hour to get their kites up, and it was arranged that observations would be made for the succeeding hour, after which they could haul them down.

It was intended to take the kites in rotation during the hour, and so, by uniform distribution of the observations, average the height maintained, and reduce the chances of a single kite having an advantage due to a puff of wind. The telephone was not quite ready for the first

observations, so the first round was taken by means of a prearranged code of flag signals so that the particular kite and the moment of observation should be known at each end. After the first round, the telephone was in order and the process of communication was much more rapid.

Four rounds of observations were successfully made on the four kites, and as each observation required the taking of four angles, sixty-four angular measurements were made in the hour. A fifth round would have been taken, but some confusion arose through a temporary stoppage of the telephone communication, and ultimately there was not time to take the fifth.

It would be tedious, and would serve no purpose to give at length all the observations and calculations, but members may like to have the more important particulars of those from which the highest point reached was determined.

The greatest observed height was attained by Mr. Brogden's kite, at the time of the third round. This was at 4h. 20m. p.m.

The observer at P found $\begin{cases} A = 28^\circ\ 19' \\ E = 28^\circ\ 47\frac{1}{2}' \end{cases}$

The observer at Q found $\begin{cases} B = 116^\circ\ 40' \\ D = 45^\circ\ 23' \end{cases}$

hence $C = 360 - (A + B) = 35^\circ\ 1'$.

The horizontal distance T between P and Q was 2147 feet, and h, the height of P above Q, was −35 feet.

In order to multiply the several factors together, as the sines or tangents have in any case to be looked out in tables, it is just as easy to look out their logarithms in the corresponding tables, and as it is merely necessary to add logarithms in order to multiply the numbers, the calculation can be wholly shown in a few lines. Thus:—

Log T	= 3·3318	Log T	= 3·3318
Log sin A	= 9·6761	Log sin B	= 9·9512
Log tan D	= 10·0058	Log tan E	= 9·7399
Log Cosec C	= 10·2412	Log Cosec C	= 10·2412
Log H	= 3·2549	Log (H − h)	= 3·2641
H	= 1798 ft.	H − h	= 1837 ft.
		h	= −35 „
		H	= 1802 „

The two determinations of the height of the kite differ by only four feet, not a great amount seeing that the kite was just over 3800 feet from the observer at P.

The mean of the two results gives exactly 1800 feet as the height of the kite above the observer at Q.

Corresponding observations of the kite station gave its height above Q as − 16 feet, that is, the the kite station was 16 feet below Q. This 16 feet has to be added to the 1800 to obtain the height attained by the kite above its own station, hence the result tabulated, 1816 feet.

The complete table of results is appended:—

HEIGHTS COMPUTED.

Number of Observations.	Kite No. 2. feet.	Kite No. 3. feet.	Kite No. 6. feet.	Kite No. 7. feet.
1	1061	1070	1523	1264
2	1105	1223	1438	1407
3	1339	1476	1816	1303
4	1250	1314	1439	1332
Mean Value:	**1189**	**1271**	**1554**	**1326**

II.—Descriptive Portion.

BY ERIC STUART BRUCE, M.A. OXON, F.R. MET. SOC.

It was thought it would be of interest to Members of the Society to show at this meeting photographs of some of the various kites used in the competition, and which were taken on the spot, and to describe some of the kites which have not already been described in our JOURNAL. In the July number of the JOURNAL there appeared a photograph of Mr. Charles Brogden's six-winged kite, which attained the highest record both as regards the mean of the observations for altitude and the actual height observed for the kites. The diagram (Fig. 1) shown makes the construction of the kite perfectly clear. Mr. Brogden's kite is really an adaptation of part of an experimental flying machine which he made some fifteen or sixteen years ago, and, to use his words, which "proved very nearly a success," as he was able to leave the level ground for distances of from 15 to 20

feet at each bound, and some 18 inches or 2 feet high, travelling like a swimmer when learning, who touches the ground occasionally with his toe. The area of the kite is 110 square feet. Mr. C. Brogden claims that it is in reality much stronger than a casual observer might think. Its very appearance of weakness is its strength, as it gives (though not too much) to any excess of pressure. The pull is remarkably constant, as the wings are so constructed that they give at their after edge, and the dihedral angle of the kite remains unchanged, as also does the

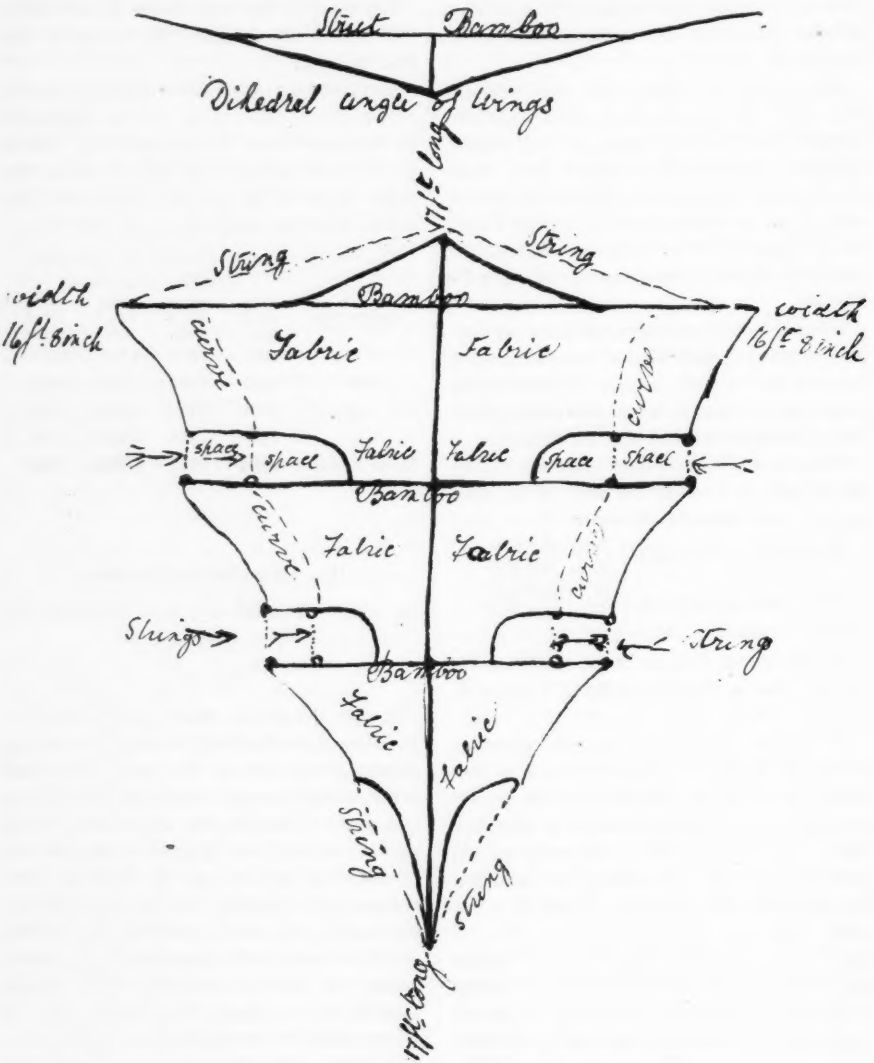


Fig. I.—Mr. Charles Brogden's Six-Winged Kite.

position of the kite itself in the air. This enables the flying wire to be attached low on the bridle, bringing the kite well facing the wind. The wings then assume the form of a bird's

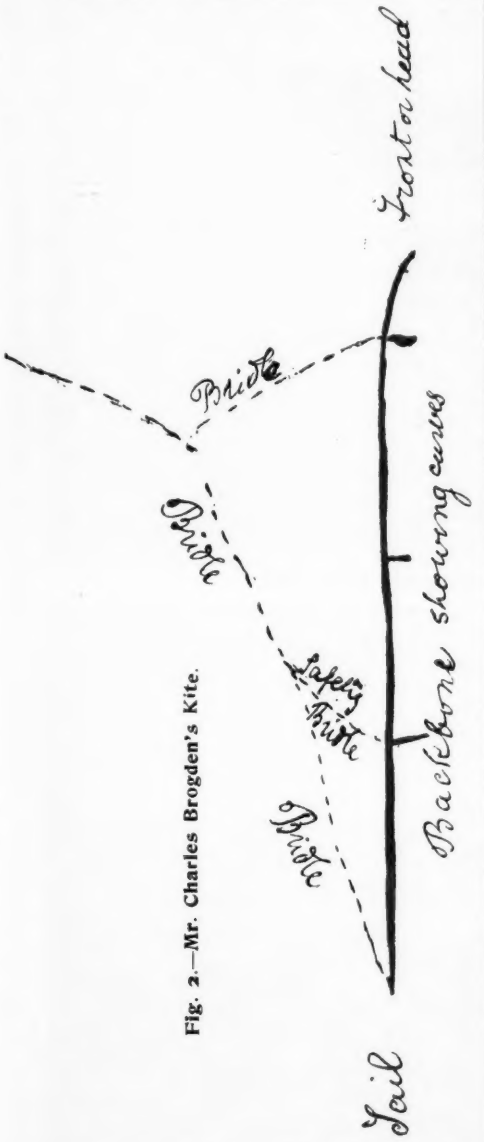


Fig. 2.—Mr. Charles Brogden's Kite.

wing, or nearly so, being not planes but curves from back to front. The form of the backbone (Fig. 2) is a feature. It is neither straight nor an even curve. The front or head end curves upward, while the after part or tail curves very slightly downward. The head or lead, which is the triangular part in front of the first pair of wings, has, therefore, a slightly upward turn, which always prevents the kite from dipping, but is not sufficient to cause much drift. The air forced under this lead spreads and fills the curves of the wings, gliding off from the back edge of these, and so tending to force the kite forward.

The dihedral angle of the kite is maintained by the struts which cross the kite above the backbone, and fix to the front rigid edge of the wings at about two-thirds of their length from the backbone.

This leaves only a third of each wing, the extreme end, in fact, to be springy, and affect the backward bend of the dihedral angle.

The free edges of the wings at the back and ends are cut *curved*, and they have strong tape sewn on so that when the strain comes the fabric blows out evenly and not in creases.

Each wing is connected from its back or free edge by strings to the front or rigid edge of the next wing. These strings are attached to loops of tape, and can be regulated as to length, so that the kite may be made to fly true always. Experiment will show at once which of these strings should be made tighter or slacker to correct any tendency of the kite to tilt a little sideways

The fixing of the bridle is of some importance; the main bridle goes from the point on the backbone where the first pair of wings are fixed, to the extreme end at the tail. This tends to keep the backbone slightly curved downward at the tail end. There is another string from the bridle to a point where the last pair of wings are fixed. This keeps the backbone from having too great a curve or even from fracture. This might be accomplished by having a backbone permanently curved and strong enough to remain so, but it has been found by experiment that a springy curve or plane seems to give the best results.

Fig. 3 shows Mr. S. F. Cody's large back kite in mid-air. This kite, as we have heard, was second in the mean of observations, and may be described as a winged box kite, having much the appearance of a huge bat. Fig. 4 shows Mr. Cody getting ready for the start, and Fig. 5 manipulating his winding gear during the competition.

Fig. 6 and Fig. 8 show Mr. S. H. R. Salmon's multiple-celled rhomboidal kite. Fig. 7, this kite just rising; Fig. 8, in the air. In this last picture you will notice the two Cody kites at a considerable height.

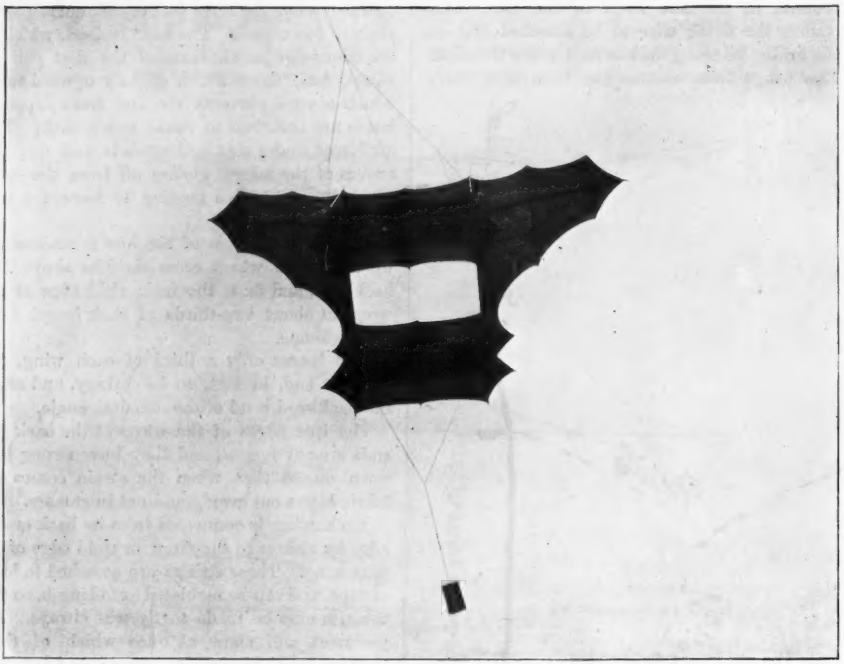


Fig. 3.—Mr. S. F. Cody's Large Black Kite.

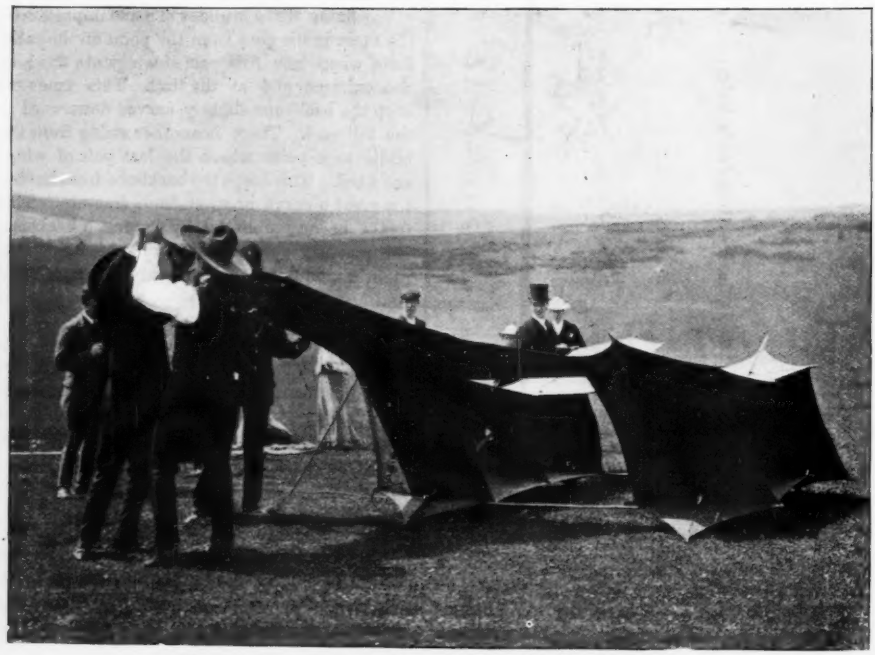


Fig. 4.—Mr. S. F. Cody getting ready for the start.

Fig. 5.—Mr. S. F. Cody manipulating his Winding Gear during the Competition.

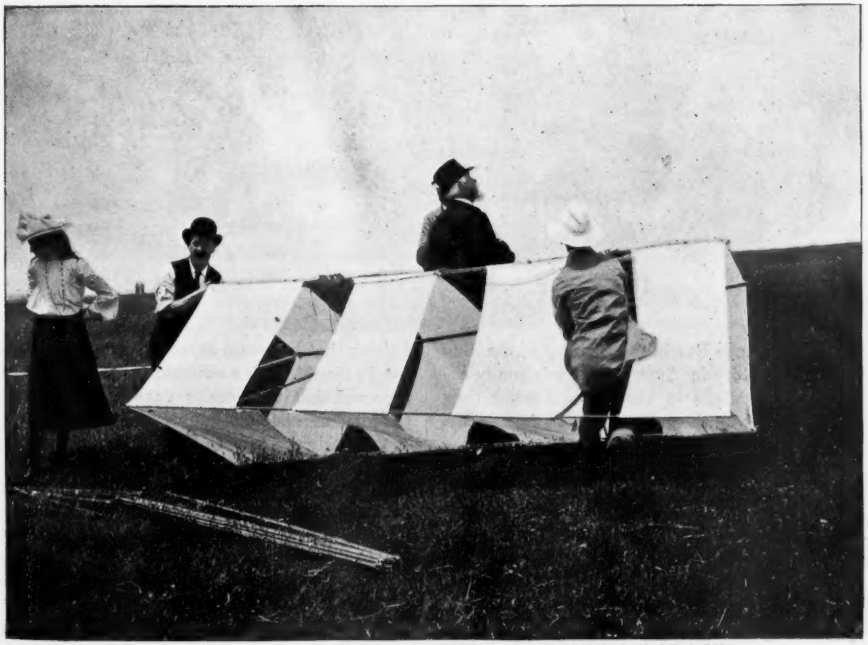


Fig. 6.—Mr. S. H. R. Salmon's Multiple celled Rhomboidal Kite.

Before closing the descriptive portion of the report of the kite competition, I should like on behalf of our Council to express our indebtedness to those distinguished gentlemen who, though not members of our Council, gave us so much of their valuable time in acting on our Jury. We also have to acknowledge our indebtedness to Mr. J. E. Dallas and Mr. N. F. Mackenzie, of the Royal Engineering College, Coopers Hill, who not only gave us their most valuable services in taking the difficult theodolite observations, but placed at our disposal the first-rate instruments that were necessary to ensure satisfactory results.

There is also a tribute of gratitude to be paid to Mr. Lawrence Rotch, the Director of the Blue Hill Observatory in America, for his many excellent suggestions to the Council when they were drawing up the general rules. And to Mr. W. H. Dines, who, though he was prevented from acting on the Jury by the fact that he was himself engaged in carrying out his important kite observations in Scotland, yet on several occasions gave us the benefit of his practical experience in kite flying.

In carrying out a somewhat difficult undertaking, requiring so much *space* for its accomplishment, we cannot too warmly express our thanks to Lord Henry Thynne and those other neighbouring landowners who so unhesitatingly, generously, and unrestrictingly placed their private ground at our disposal for the competition.

The Chairman: Ladies and Gentlemen,—It is not the custom to discuss a report read at this Society, but I would like to say that when Mr. Boys is expounding a thing it always seems so remarkably easy, and the way in which he conducted you through the process of determining the height of a kite may give you the impression that you have only to go out with a theodolite and the thing is done. But as a matter of fact, I think that he would agree with me that he derived a considerable amount of satisfaction when it proved that the results gave the heights of the kites unmistakably to within a foot or two. It is not always so with theodolite observations. There is a well-known story told at Cambridge of a teacher who wanted to illustrate the use of a theodolite in determining the height of a building, and he went out and took observations of the King's Chapel, and after going through a number of calculations involving four figures he arrived at one foot decimal some fraction as the height of the chapel. So it is not all done

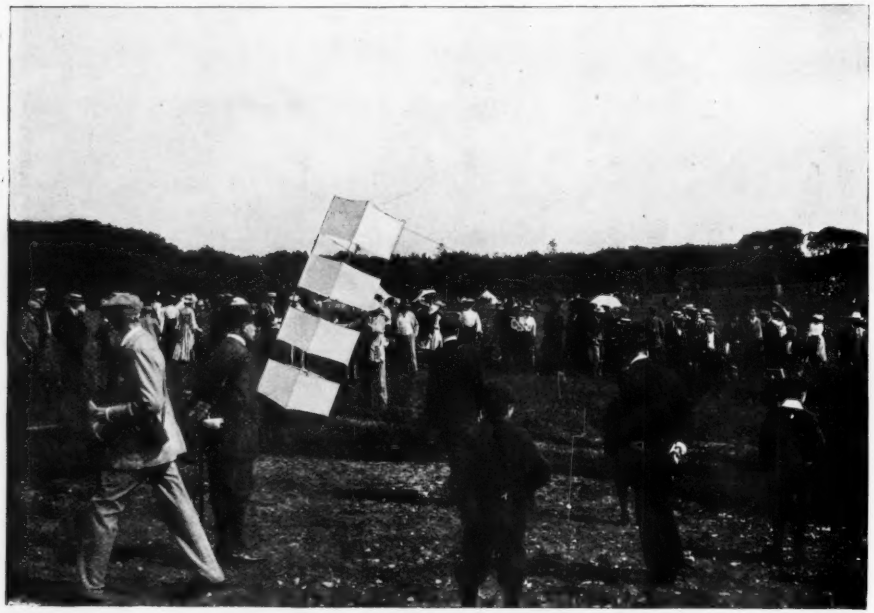


Fig. 7.—Mr. S. F. Salmon's Kite just rising.

when you go out with a theodolite, and I think I may offer the congratulations of the Society to those who were instrumental—Mr. Boys and the Hon. Secretary—in carrying on this competition under such difficult conditions and to such a successful issue. It is a pity the kites did not reach a sufficient altitude for the medals to be awarded, but that was not the fault of the Society or of the Aëronautical Committee. I wish here to interpolate a matter that will not take long, but it is not a small matter. Among the apparatus carried out by the National Antarctic Expedition on the *Discovery* was a balloon equipment, and the equipment was carried, I believe, at the suggestion of this Society, no doubt with the goodwill of Commander Scott, who is himself a member of this Society. Well, Lieutenant Shackleton, who was engaged on the Expedition, came back in the *Morning*, and is present to-night, and will be able to give the Society some information about the ballooning part of the work of that Expedition. That Expedition promises very magnificent results, and we shall all be interested to hear something about the balloon work, and I will now ask Lieutenant Shackleton to say a few words on the subject.

The Use of the Balloon in the British Antarctic Expedition.

By LIEUT. E. H. SHACKLETON, R.N.

Mr. Chairman, Ladies and Gentlemen,—After the learned discussion to-night, I approach this rather as an amateur, so you must not expect much in the way of mathematics. I believe the balloon was got for the ship at the suggestion of this Society, and it was owing to two ladies who took great interest in the matter that we were able to purchase it. Before the ship went, I was chosen to be the aëronaut, and with four men I went to Aldershot to undergo a course of training in ballooning. We did not know very much about it, and we only had a week to learn. But we had one sailor there who was a good man, but he did not like going up at first, and when the instructor said you go ahead, you go up in the net, he first said, "I'd rather go in the car," and then he said, "I think I will go in the net, because the man in the car will strike the ground first if it comes down bump." We went up and eventually landed in a rose garden, amid strong language and a gardener. Another time we landed in a field of oats. I

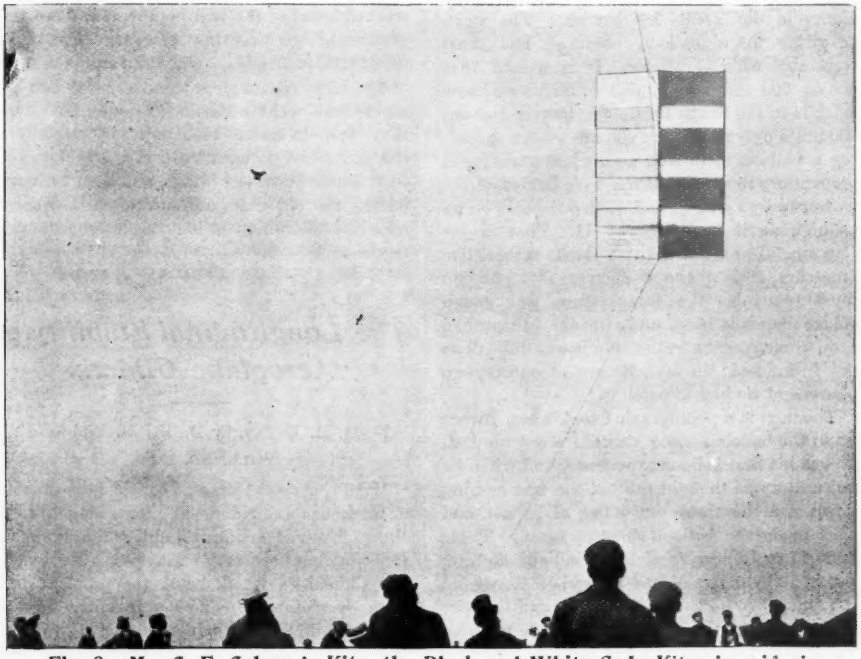


Fig. 8.—Mr. S. F. Salmon's Kite, the Black and White Cody Kites in mid-air.

had on a very nice suit, and it was all torn and made a mess of, and I said to the most important looking rustic, "What damage have we done?" And he said, "The oats are all right," and I said, "Are they yours?" And he said, "No, they bain't mine." The clothes of one man were so damaged that when he got back he said he was going to write a letter to his girl, and I asked him what for, and he said he was going to tell her to go on darning all the socks she could find. We had a week of this sort of work. Now we will pass to our work in the Antarctic, and I will show you a photograph of our ship in latitude 78° 50′ South.

The first picture shows the "Discovery" lying alongside the great ice barrier. The next, preparing for a balloon ascent. You must remember this is all ice. It is a cliff that is over 500 miles long, and so far as we know extends to the South Pole. We travelled about 300 miles over it. There you see we are spreading a balloon cloth and we are just unrolling it preparatory to sending it up. We landed all the hydrogen by 9 o'clock and we had the balloon up as high as it would go by 11. That is the Captain. I am afraid we look rather like mateurs getting the balloon ready, but you must remember the temperature was much colder than it is here, and after the balloon had been three months in the ship it was difficult to get it inflated; in fact, it tore in one or two places and we had to patch it.

The next is a photograph I took when I went up in the balloon. The Captain went up first. It was his first balloon experience, and when he got well up he thought the balloon was coming down, and he threw out a bag of ballast, and that made the balloon shoot up again. There were 17 or 18 degrees of frost, and although we put in 19 cylinders of gas instead of 16, we did not even then fill the balloon. We had the wire a little bit on the south side, because we were right over the sea, and the wind was blowing towards the north.

The next photograph I took from a height of 750 feet. These are men, and those are their shadows.

In the next picture the deflation of the balloon is seen. The wind began to blow, and we had to take the balloon down in a great hurry.

Those are the only pictures I have to show you. But I believe this is the first time a balloon has been up within the Antarctic circle. I do not know what André did. We put it up in the midst of that great plain of ice, half way in the middle, between the new land we found to the east and the old land we found to the west, and the reason for putting it up there was to see if there was any view to the south. From a height of 750 feet with the glasses you could see, and if it had not been for our getting frozen, no doubt we would have used the balloon more. I feel sure that any future expeditions will always take a balloon with them. I only once heard of a place in the Polar regions where a balloon would have been useful. It appears there were two Esquimaux families living close to one another, one was a young man and the other was an old man, whom the young man regarded as his enemy. He had a pretty daughter, and the young man wanted to run away with her. Now owing to the extreme cold a great cavern cracked between the two houses, and there was a snow bridge over the cavern. This fellow thought this will be all right, I can cross the snow bridge and we can elope; and so he waited till night-time, went across the bridge—you know they sleep in sacks—took up a sack containing the girl as he thought, crossed the bridge, and then broke down the bridge, and then he found it was the old man, and not the girl. That is all I can tell you about balloons in the Antarctic regions.

The Longitudinal Stability of Aëroplane Gliders.

By G. H. Bryan, Sc.D., F.R.S., and W. E. Williams, B.Sc.

1. During recent years, a considerable amount of work has been done in attempting to solve the problem of artificial flight, and in the main this work has been very successful. One by one the difficulties which beset the problem have been overcome, and now apparently there is only one difficulty which stands between us and the solution of the problem—namely, the difficulty of balancing the machine. In the following paper an attempt is made to further the solution of this difficulty, by showing that it is capable of mathematical treatment.

We have not attempted to deal exhaustively with the question; we have confined ourselves to the consideration of simple forms of gliders, and the chief object of the paper is to draw the attention of all interested in the problem of artificial flight to the capabilities of this method.

In thus urging the importance of mathematical investigation, we do not lose sight of the necessity for experimental work, but the results obtained by experiment will be much

more valuable if there is a mathematical theory to direct the course of the experiments, and to correlate the results obtained.

We must first define "stability of a mode of motion" as applied to flying machines, and show how it can be expressed mathematically. We may say that the motion of a flying machine is *steady* when the resultant velocity is constant in direction and magnitude, and when the angle of the machine to the horizontal is constant. If this motion is slightly disturbed, the machine may either return after a time to the original motion, or it may take up a new and altogether different mode of motion. In the first case the steady motion is said to be stable, and in the second, unstable.

It is evidently necessary for steady motion of any kind that there should be equilibrum—*i. e.*, that there should be no forces acting on the machine (apart from accidental disturbances), which tend to vary the motion, and hence it follows that the number of modes of steady motion of which a machine is capable is, in general, limited, and that when an unstable steady motion is disturbed, the new mode of motion taken up is entirely different from the old.

It is necessary to distinguish carefully between equilibrium and stability, as the two are very often confused together. Equilibrium is necessary to secure the existence of a mode of steady motion, but is not sufficient to ensure the stability of the motion.

The question of the stability of a rigid body moving under the action of any forces has been solved by Routh. In order to apply his results to the stability of flying machines, it is necessary to know the moment of inertia of the machine about its centre of gravity, the resistance of the air on the supporting surfaces as a function of the velocity and angle of incidence, and also the point of application of this force, *i.e.*, the centre of pressure for different angles of incidence. If these are known for the surfaces constituting any machine, then the problem of its stability for small oscillations can be completely solved. Unfortunately, our knowledge of these points is very unsatisfactory. Several valuable series of experiments have been made to determine the resistance on planes, but there is still some doubt as to the position of the centre of pressure at small angles of incidence, especially for oblong planes, and very little indeed is known as to the movement of the centre of pressure on concave surfaces. Until experiments are made on this point it will be impossible to solve the problem of stability for machines supported on concave surfaces.

2. Let us consider any system of aëroplanes having a plane of symmetry, and let it be descending with this plane of symmetry vertical. To specify the motion, which we suppose to be always in the same vertical plane, choose

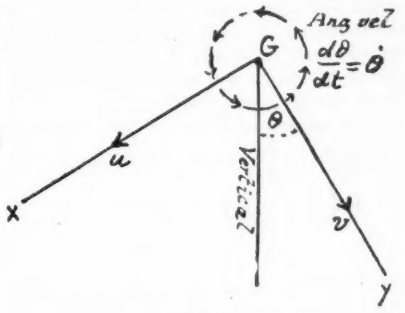


FIG. 1*a*.
SPECIFICATION OF VELOCITIES.

as axes of reference two lines at right angles Gx, Gy. (Fig. 1*a*) fixed in the body and passing through its centre of gravity. Then the motion will be completely determined if at every instant we know—

(1) The angle θ which one of the axes—say, the axis of y—makes with the downward drawn vertical.

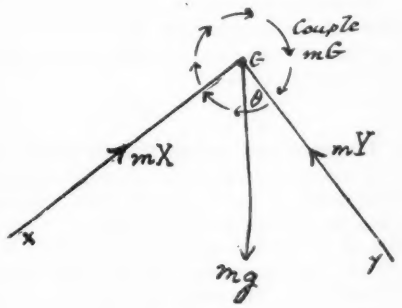


FIG. 2*a*.
SPECIFICATION OF FORCES.

(2) The velocity components of the body along the two axes, which we shall call u, v.

The angular velocity of rotation of the body about its C.G will be $\frac{d\theta}{dt}$ or $\dot{\theta}$. We shall use m to

denote the mass of the body, mk^2 its moment of inertia about its centre of gravity.

The resistances of the air on the several parts of the system will in general be functions of the velocity components u. v. $\dot{\theta}$. By the laws of statics these resistances are always equivalent to two forces along the axes of reference and a couple about the origin. For convenience, we shall denote the forces by mX and mY, and the couple by mG. So that X, Y, G are respectively equal to the forces and couple divided by the mass of the body. (Fig. 2*a*.)

The equations of motion of the system are

$$m\left(\frac{du}{dt} - v\frac{d\theta}{dt}\right) = mg \sin\theta - mX$$

$$m\left(\frac{dv}{dt} + u\frac{d\theta}{dt}\right) = mg \cos\theta - mY$$

$$mk^2 \frac{d^2\theta}{dt^2} = - mG.$$

If the motion is to be steady, then these equations must be satisfied when u, v, θ are constants. Putting the time differentials equal to zero we have the conditions of steady motion.

$$O = g \sin\theta - X$$
$$O = g \cos\theta - Y$$
$$O = - G$$

If now the resistance of the air on the surfaces of the aëroplane be known, X, Y, G may be expressed as functions of u. v. $\dot{\theta}$. In the particular case considered moreover $d\theta/dt = o$. Equations (2) therefore become three equations involving u. v. θ. The values of u. v. θ given by these equations of motion completely determine the steady motion.

The resultant velocity is equal to $\sqrt{u^2 + v^2}$. This velocity makes with the axis of y an angle $\tan^{-1}(u/v)$, and therefore it makes with the vertical an angle

$$\left(\tan^{-1}\frac{u}{v} - \theta\right)$$

3, Fluctuations about Steady Motion.

To ascertain whether the system is longitudinally stable, we must examine what happens when the system, descending steadily as considered in the last paragraph, is slightly disturbed. Let us suppose that the effect of the disturbance is represented at time t by small increases δu δv in the velocity components and an increase $\delta\theta$ in the angular coordinate. Let u_0 v_0 θ_0 be the values of these coordinates corresponding to steady motion, so that in the disturbed motion we have, $u = u_0 + \delta u$, $v = v_0 + \delta v$, $\theta = \theta_0$ $\delta\theta$. Also let X_0 Y_0 G_0 be the forces and couple in the steady state, then in the disturbed state we know that neglecting the second order of small quantities these may be expanded in the form

$$X = X_0 + \delta u \frac{dX}{du} + \delta v \frac{dX}{dv} + \delta\dot{\theta} \frac{dX}{d\dot{\theta}}$$

and similar expressions for Y and G.

For brevity we shall denote differential coefficients such as $\frac{dX}{du}$ ... by X_u ...

Let us now substitute these values in the equations. By reason of the conditions for steady motion, the terms independent of δu δv $\delta\theta$ vanish, and we are left with

$$\frac{d}{dt}\delta u - u_0 \frac{d}{dt}\delta\theta = \cos g\ \theta_0\ \delta\theta$$
$$- X_u\,\delta u - X_v\,\delta v - X_{\dot{\theta}}\,\dot{\delta\theta}$$

$$\frac{d}{dt}\delta v - v_0 \frac{d}{dt}\delta\theta = - g \sin\theta_0\ \delta\theta$$
$$- Y_u\,\delta u - Y_v\delta v - Y_{\dot{\theta}}\,\dot{\delta\theta}$$

$$k^2 \frac{d^2}{dt^2}\delta\theta = - G_u\,\delta u - G_v\,\delta v - G_{\dot{\theta}}\dot{\delta\theta}$$

To solve these quotations we put

$$\delta u = P\epsilon^{\lambda t} \quad \delta v = Q\epsilon^{\lambda t} \quad \delta\theta = R\epsilon^{\lambda t}.$$

If we make these substitutions and afterwards eliminate the unknown quantities P, Q, R, we obtain an equation of the fourth degree in λ, which may be written

$$A\lambda^4 + B\lambda^3 + C\lambda^2 + D\lambda + E = O$$

where A, B, &c., are expressions involving k^2, θ_0, u_0 . . , X_u . . . G_u, &c.

4. This equation in λ is the period equation for the small oscillations of the system about the state of steady motion, and the nature of its roots determines the nature of the small oscillations of the system, and in particular determines whether the system is stable or not. If λ_1 λ_2 λ_3 λ_4 be the four roots of this equation, then the value of any co ordinate (say, y) affected by the small oscillation at any time, t, will be given by an expression of the form.

$$y = y_0 + a_1\epsilon^{\lambda_1 t} + a_2\epsilon^{\lambda_2 t} + a_3\epsilon^{\lambda_3 t} + a_4\epsilon^{\lambda_4 t}$$

where a_1, a_2, a_3, a_4 are constants depending on the initial conditions, and y_0 is the value of the coordinate in the steady motion. Thus we may take y to represent the distance of the centre of gravity from the line of steady motion. If, further, we take x to represent the distance travelled along this line, then a curve with y as ordinate and x as abscissa will give the actual path of the centre of gravity after a small disturbance. The form of this path depends on the nature of the roots λ_1 . . . λ_4 Let us suppose first that λ_1 is real and positive, then the part of y corresponding to it will be

$$y = a_1\epsilon^{\lambda_1 t}$$

Since λ_1 is real and positive y increases continually with the time, that is to say the centre

Fig 1 Curve showing $y = e^{\lambda t}$ λ positive

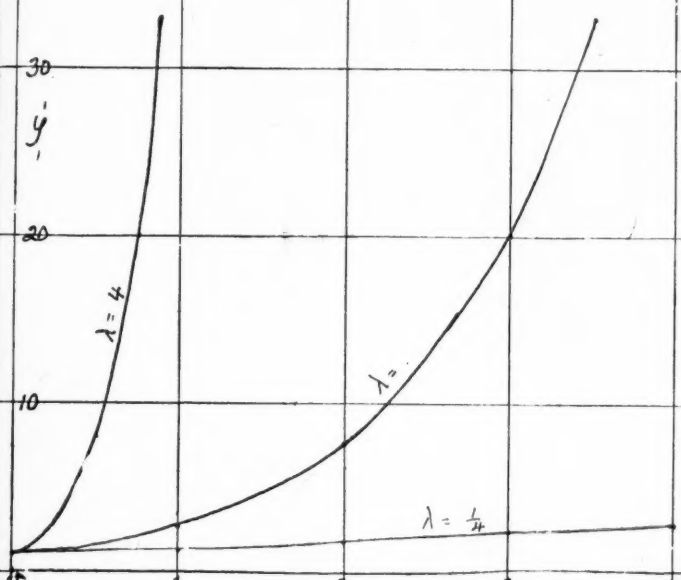


Fig 2 Curve showing $y = e$

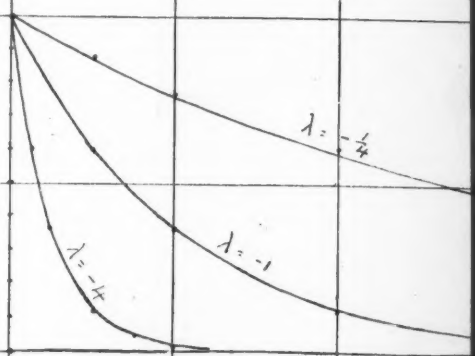
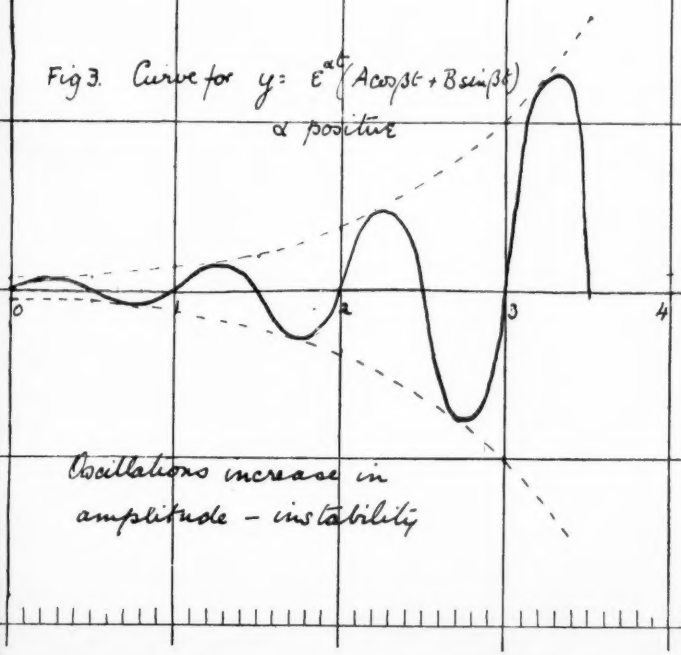
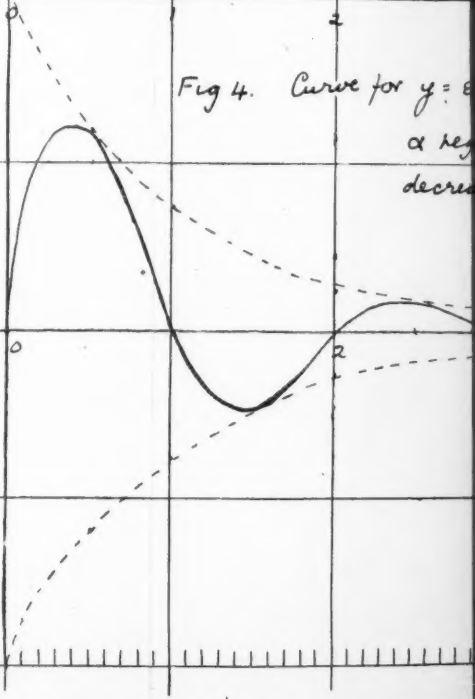


Fig 3. Curve for $y = e^{\alpha t}(A\cos\beta t + B\sin\beta t)$
α positive



Oscillations increase in amplitude – instability

Fig 4. Curve for $y = e$
α neg
decre



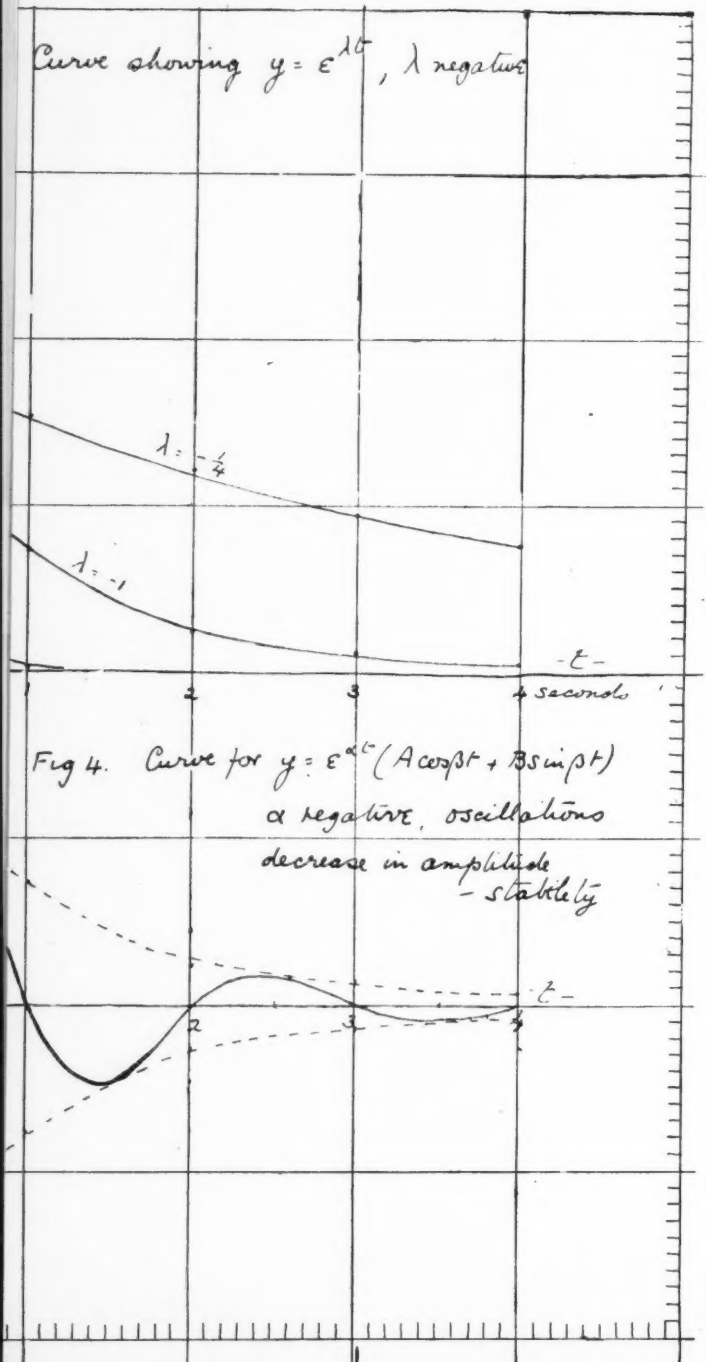


Fig 4. Curve for $y = \epsilon^{\alpha t}(A\cos\beta t + B\sin\beta t)$
α negative, oscillations decrease in amplitude - stability

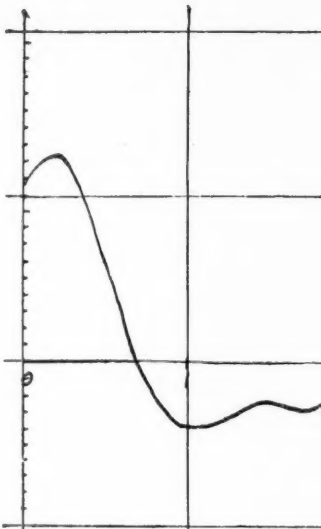


Fig 5. Composition
increasing and

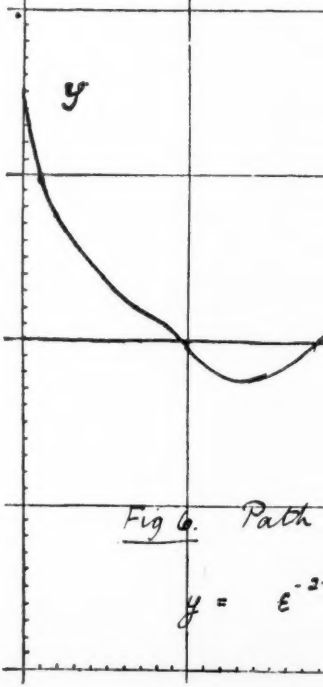
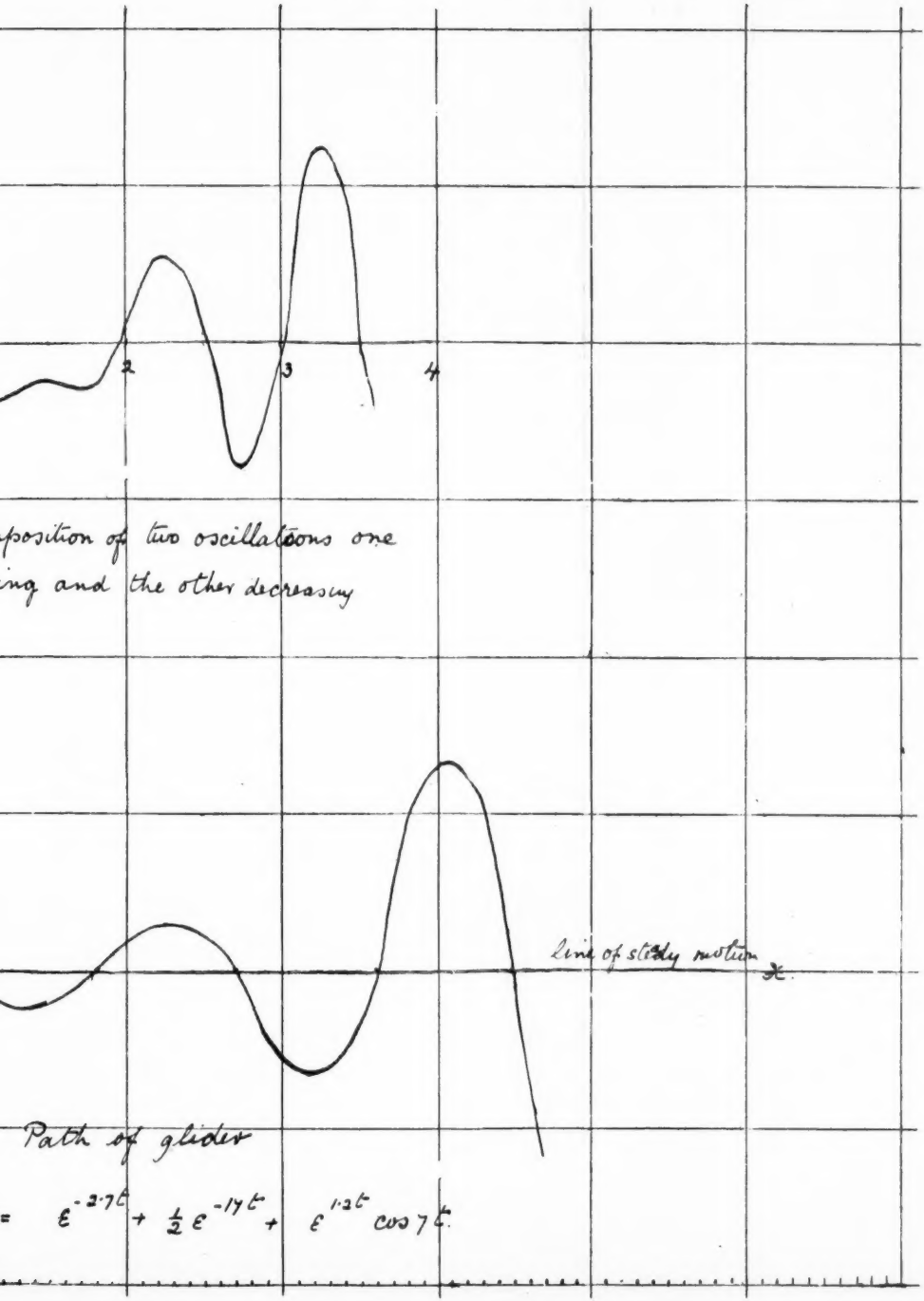


Fig 6. Path

$y = \epsilon^{-2}$



position of two oscillations one
ing and the other decreasing

Path of glider

$= \varepsilon^{-2.7t} + \frac{1}{2}\varepsilon^{-1.4t} + \varepsilon^{1.3t}\cos 7t$

of gravity moves away from the line of steady motion, and the motion is therefore unstable.

If, however, λ_1 is real and negative, $\epsilon^{\lambda_1 t}$ will decrease continuously with the time, and the centre of gravity will continuously approach the line of steady motion, so that as far as this root is concerned the motion is stable. The curves representing the equation $y = c\epsilon^{\gamma t}$ are drawn in Figs 1, 2. We see that the greater the numerical value of the root, the more rapidly does the value of y increase or diminish.

Let us next suppose that one of the roots be complex, *i.e.*, that it contains an imaginary quantity. Then since the coefficients of the equation are real, it is necessary that there should be another complex root of the same numerical value, but of different sign. Let the roots be

$$\alpha + i\beta \text{ and } \alpha - i\beta$$

where $i = \sqrt{(-1)}$, then we have

$$y = a_1 \epsilon^{(\alpha + i\beta)t} + a_2 \epsilon^{(\alpha - i\beta)t}.$$

This may be written in the form

$$y = \epsilon^{\alpha t} (c_1 \cos \beta t + c_2 \sin \beta t).$$

The curve corresponding to this equation is plotted in Figs. 3, 4. It consists of a series of waves representing oscillations of the glider about the mean position for steady motion. If α, the real part of the root, is negative, the oscillations continually decrease in amplitude and finally disappear, but if α is positive the oscillations continually increase. In the first case the motion is stable, in the second it is unstable and will ultimately break down, the glider beginning by pitching dangerously and finally taking up some other mode of motion. In plotting the path of the glider we must take x instead of t as abscissæ, but to a first approximation we have $x = Vt$, since the variations of the velocity V may be disregarded in comparison with its value for steady motion.

The curves so obtained will differ from those shown in Figs. 3—6 in that the abscissæ will be lengthened out in the ratio V : 1. By adding together the ordinates corresponding to the four roots $\lambda_1 \lambda_2 \lambda_3 \lambda_4$ we get a curve representing the actual path of the centre of the glider. A few such curves are shown in Figs. 5, 6.

5. In order that the steady motion should be stable it is necessary that the oscillations corresponding to every root should diminish. The condition of stability is therefore that "the real roots and the real parts of the imaginary roots of the period equation should be negative."

It has been shown by Routh that this condition may be very simply expressed in terms of the coefficients A, B, C, D, E.

In order that the real parts of the roots should be negative, it is necessary and sufficient that the coefficients A, B, C, D, E, and the expression $BCD - AD^2 - EB^2$ should all have the same sign. In our case A is essentially positive, being equal to k^2, and therefore the condition of stability is that the quantities B, C, D, E and $BCD - AD^2 - EB^2$ should be positive.

In order to ascertain by calculation whether any particular kind of steady motion of a glider is longitudinally stable, the following steps would have to be adopted:

(1.) The moment of inertia of the glider about its centre of gravity ($m k^2$) and the nine quantities which we have called $X_u \ldots\ldots G_{\dot{\theta}}$ would have to be determined by experiment combined with calculation.

(2.) The five coefficients A, B, C, D, E would have to be expressed in terms of the quantities determined under the first step, and the quantity $BCD - AD^2 - EB^2$ then calculated. Calling this last quantity H, we should know that if B, C, D, E, and H were positive, the steady motion under consideration would be stable; in other words, it would be safe to perform gliding experiments at the particular angle of gliding and with the particular velocity for which the various quantities had been calculated. If any of the quantities were to be found negative, we should know that the steady motion in question was unstable; in other words, that the glider could not travel steadily in the manner proposed but would be certain to overturn sooner or later.

The expressions for A, B, C, D, E being somewhat long, we do not propose to discuss their general forms until the methods for calculating them have been reduced to the simplest possible forms. Considerable simplifications are possible in the following cases:—

(a.) When the gliding machine and its supporting aëroplanes or aërocurves are perfectly rigid and the air resistances are assumed to be proportional to the square of the relative wind-velocity. This case will be discussed fully in a subsequent paper.

(b.) When the supporting surfaces are flat and all lie in the same or parallel planes, and there is no tangential resistance. In this case the system will only glide steadily when the aëroplanes are horizontal, and taking their direction as the axis of y, we shall have $\theta_0 = 90°$. and Y and all its differential coefficients will vanish. The quantities A, B, C, D, E then take the comparatively simple forms:

$$A = k^2 \qquad B = k^2 X_u + G_{\dot\theta}$$
$$C = v_o G_u - u_o G_v - X_{\dot\theta} G_u$$
$$D = u_o (X_v G_u - X_u G_v) - g G_v$$
$$E = g (X_v G_u - X_u G_v).$$

6. We will now proceed to apply these conditions to the motion of a simple form of glider.

Let us take a single square plane lamina with its centre of gravity in its plane and balanced so as to fly at an angle of 10°, and suppose that the moment of inertia about the centre of gravity is $\frac{1}{2} ma^2$ where 2a is a side of the square. Let the resistance of the air to the lamina be given by the expression

$$R = K S V^2 f(\alpha)$$

where S is the area of the plane, V the velocity, and α the angle of incidence, in this case 10°. Then from Langley's tables we find

$$f(\alpha) = \cdot 3, \frac{df}{d\alpha} = 1 \cdot 6$$

and if we represent the couple G due to the air resistance by $K S V^2 f(\alpha) \times a\phi(\alpha)$, Joesel's formula gives

$$\phi(\alpha) = \cdot 49, \frac{d\phi(\alpha)}{d(\alpha)} = - \cdot 59.$$

From these data we may calculate the values of the coefficients $X_u, \ldots G_{\dot\theta}$.

We have

$mX_u = 1 \cdot 66$ KSV $\quad mG_u = \cdot 18$ KSVa
$mX_v = \cdot 31$ KSV $\quad mG_v = - \cdot 03$ KSVa
$mX_{\dot\theta} = \cdot 73$ KSVa $\quad mG_{\dot\theta} = \cdot 09$ KSVa^2

Substituting these values in the expressions for the coefficients given above, assuming that $k^2 = \frac{a^2}{2}$, and dividing through by m^3 we obtain

$$A = \frac{a^2}{2}$$

$$B = 92 V$$

$$C = 18 a - 1340 \frac{a^2}{V^2}$$

$$D = 295 \frac{a}{V}$$

$$E = 36400 \frac{a}{V^2}$$

and $\therefore BCD - AD^2 - EB^2$

$$= \frac{a^4}{V^2} \left\{ 445{,}000 - 344{,}500{,}000 \frac{a}{V^2} \right\}$$

All these quantities will be positive and the glider will therefore be stable if V^2 is greater than 774a. Thus a glider of breadth one foot will be stable for all velocities above 20 feet per sec.

If we assume any particular values for a and V we may substitute them in the period equation which may then be solved. Thus let us assume,

$$a = 1 \text{ foot} \qquad V = 10 \text{ feet per sec.}$$

The equation becomes

$$\lambda^4 + 18\lambda^3 + 4 \cdot 6\lambda^2 + 29 \cdot 5\lambda + 364 = 0$$

This equation has two real and two imaginary roots, namely

$$- 2 \cdot 7, \quad - 17 \cdot 7 \quad \text{and } 1 \cdot 2 \pm 7 \sqrt{-1}$$

The imaginary roots correspond to an oscillation of gradually increasing amplitude indicating instability.

The period of the oscillation is about ·9 sec., and the length of a wave on the path is therefore 9 feet.

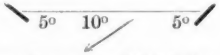
(The path would be something like Fig. 8)

7. The conditions of stability have been investigated for a number of different forms of gliders and the following results have been obtained—

(i) Square plane gliding at an angle of 35° and having $k^2 = \frac{1}{2}a^2$

Stable if $V^2 > 3000a$

(ii) Two very narrow equal slats, one behind the other (Fig. 7), inclined to the line joining centres at angles of 5° and − 5° and flying at an angle of 10° Distance between slats = 3a, assumed value of $k^2 = 2a^2$



Stable if $V^2 > 590a$

(iii) Two equal square planes, one behind the other, set at angles of 5° and − 5° and flying at an angle of 10° as in last case. Side of each square = 2a, distance between centres = 5·4a. and value of $k^2 = 4a^2$.

Stable if $V^2 > 2000a$

where a is the side of the square, and is equal to the distance between the planes, the moment of inertia being

(iv) Two unequal square planes, areas 10 : 1, with small plane tilted up.

(*a*) Small plane in front at an angle of 10° to large one. Direction of motion making angles 10° and 20° with large and small planes. Sides of squares 2a and 2a/3·1. Distance between centres = 3a. Assumed value of $k^2 = a^2$.

Stable if $V_2 > 1040a$,

(*b*) Small plane behind at an angle 5° to large one. Direction of motion making 10° and 5° with large and small planes. Dimensions and distances of planes and moment of inertia about C.G. as before.

Stable if $V^2 > 250a$.

8. From these results it will be seen that gliders made of flat planes are stable if their velocity exceeds a certain quantity depending on the dimensions and form of the glider. It is obvious

that the lower this velocity the more satisfactory will the glider be for practical purposes. Of the forms we have calculated, this velocity is lowest for a square plane with another small plane attached behind. In experimenting with small gliders it is found that gliders made of single planes are very difficult to start properly, and that gliders made of two planes set at an angle have a very great advantage in this respect. The reason for this is not far to seek. A glider made of a single plane has a mode of steady motion besides that of proper flight, namely a motion in which the lamina moves head first with angle of incidence of zero. This motion is, of course, unstable, but if the plane is balanced for small angles this motion may persist long enough to wreck the glider. If, however, the glider has a tail at an angle to the main aëroplane, or if it is made up of two planes making an angle with each other, the vertical motion is no longer one of equilibrium, and cannot be taken up.

Many of the gliding experiments which have been made by Lilienthal, Chanute, Pilcher, and others may not improbably have been made with machines which were unstable, and were only kept balanced by the skill of the aëronaut. In order that this should be possible, it is necessary that the departure from steady motion should be slow, so that the experimenter may have time to move so as to preserve the balance. This requires that the real parts of the roots of the period equation, if positive, should be small, and the smaller they are the easier will it be to keep the machine balanced.

So far we have confined our attention to gliders falling under gravity. An almost exactly similar calculation would, however, apply to motor-driven machines. On reference to §3 it will be seen that the terms independent of δu δv $\delta\theta$ disappear from the differentiated equation. The force of the motor would come under this head, and, therefore, it would only affect the stability in so far as it affected the constants of the steady motion, and the nine coefficients X_u . . . $G_{\dot{\theta}}$. Since the propulsive force of the motor would in general be approximately a function of the velocity of the air relative to the screw propeller, the portions of these nine coefficients representing the effects of the motor would have to be determined by experiments.

The Chairman: We shall be glad to hear if anyone has any remarks to make upon this very recondite paper of Professor Bryan.

Professor Boys: I should just like to say that I have listened to this paper of Professor Bryan's with a kind of mixed interest, mixed in this sense, that it was obviously and manifestly impossible in a discourse giving just an outline of what he has been working out to make the processes he has been following in their detail-able to be followed. The problem of the stability of the aeroplane is manifestly a thing of the utmost importance. I mean, of course, the importance of it is a matter which has been very well known and appreciated for a long time, but the question as to how the determination of that stability should be attacked successfully has up to the present remained incomplete. We have, I am sure, to thank Professor Bryan. Everyone who is interested in this problem must be thankful to Professor Bryan for bringing his mathematical knowledge to bear upon the great difficulties this problem presents. Professor Bryan, I remember, was at Sir Hiram Maxim's place when he had the big machine on the track there, and I do not know whether he is aware that Sir Hiram was able, by means of a most elaborate system of dynamometers, to measure the pressure and velocity on every one of the wheels by which the thing was held up, and also held down. He determined, I believe, all the constants that Professor Bryan desires to have, and I think it is quite probable if he has not already asked for them, that if he were to ask, he could get a whole host of figures enough to keep him quiet for a very long time. It may be he has already asked for and got all he wants. This question of stability is one, of course, of the very greatest importance, not only in this subject, but in all engineering. Stability is at the root of success, and the student who begins to understand a little bit about the nature of apparatus is always ready to design some beautiful way of doing a thing, until he is brought up by this everlasting ghost—it is not a ghost, it is far too real—of stability. The knowing how to calculate the stability in any case that arises, no matter what the experiment is, is one of the most essential elements of success. Professor Bryan referred to the egg not standing up as a familiar example of an unstable position. An egg won't stand up on its end. When he said that, he reminded me of an old question used at an examination at one of the universities: Give three reasons why a pin will not stand upon it's point? And the Examiner received this answer from one of the candidates. The three reasons why a pin will not stand upon its point are as follows:—A point is that which has no parts and no magnitude; a pin, therefore, cannot stand upon that which is not; secondly, a pin will not stand upon its head, much less

will it stand upon its point; and thirdly, but it will, if you stick it in. And that brings me to the concluding remarks I wish to make about but it will if you stick it in. I don't altogether agree with Professor Bryan in his apparent desire to stifle all experiments. Yes, I agree with him if a foolish man wants to go up and kill himself, especially if he is going to kill other people. But all experiments, if Professor Bryan or other competent mathematician is not at hand, should not be stopped. Let the experiment go on. Very often the inventor is a man who has merits and ideas, but cannot explain them, but who by his genius stumbles upon an extraordinarily perfect thing, we will say without understanding, but he does it. And if these people who have that gift are to be stopped until Professor Bryan has concluded all his calculations, and, possibly, like our friend at Cambridge, brought out that King's College Chapel is one foot high, it will be a great mistake. Even the mathematician might be wrong. There are such things as mathematical errors. Don't let the practical man be stopped. The real thing seems to me to be that the two men should work hand in hand and be always ready to assist each other.

The Chairman: If no one else wishes to say anything about this paper, I would like to enforce what Professor Boys has said. I don't think it is desirable to suggest that the people who are engaged in experiments should give them up and take to mathematics. From the mathematics Professor Bryan was good enough to read us, I have a sort of impression that it will be fair to say that the same people are not entirely suitable for both those occupations. The experiment and the mathematical calculations are not things that can be taken alternatively by the same person, and to some experimentalists if you were to present the alternative of doing Professor Bryan's mathematics or facing the risk of an air-ship without them, I believe they would take the risk of the air-ship. At the same time, I think the Society ought to be extremely grateful to Professor Bryan for bringing this matter forward, because the results are really wonderful. I mean the attempt to reduce the motion of a glider to mathematics. That anybody should get so far, and be so successful with it as Professor Bryan has been, is a matter for congratulation. Experiments with gliders are very easy. If you take an ordinary pack of playing cards and try to toss them from one end of the room to the other, it is easy enough to toss half the pack. About half the pack would go to the wall and half the pack would drop on to the floor. The conditions to which they were subjected would be exactly the same so far as you can tell, and yet one lot would go without any intention of stopping and the other would become unstable and tumble on the floor. These are mysterious matters, and the fact that Professor Bryan has applied mathematics to them, and is getting out some results which are clearly of extreme practical value is a matter of great congratulation. I should like to thank Professor Bryan for his interesting remarks. I don't know whether he has anything more to say.

Professor Bryan: I do not wish for a moment to undervalue the experimental side of the investigation. What I do wish to call attention to is the futility of making random experiments, I do hope that in a future paper it may be possible to indicate the lines on which experiments should be made for determining the coefficients on which the stability of an aerial machine depends. I do hope it will be ultimately possible to put into the hands of the experimentalist the mathematics reduced to the form of the simplest possible rule for testing the stability of aerial machines, and that when that has been done, the glides will be made with full sized models, and that photographs of these will be taken, and that the whole subject will be dealt with experimentally in connection with the mathematical theory. As regards what has been said that there are some who do mathemathics and others who do the experiments, I believe that that is the case; but unfortunately, the man who is fit to do the experiments does not as a rule give a mathematician very much thanks for his mathematics, and there is no provision for consulting mathematicians in this country, as I hope there will be at some time, because I believe it will be very useful, and the experimenter as a rule wants to get all the patent rights himself in any machine, and does not give the mathematician a look in. That is the position of things in this country, consequently the only way that is open to the mathematician to help forward in any advance is by educating the experimenter up in mathematics. You will find through the whole of the modern movement, everything is for teaching mathematics to engineers and not of putting engineering problems in the hands of the mathematician. Such being the case, there is only one way of getting forward, namely, that the engineer will expect the mathematician to educate him in the problem of longitudinal stability, or he will go tumbling on in his own way and possibly may light on results, and possibly not. I hope

the results will be to bring theory and practice into closer agreement. But I have pointed out the difficulty which exists in this country in any application of mathematics to any branch of experimental science. And there is also the general form of scepticism on the part of the engineer who thinks that may be all very well, but he would rather leave things to be a matter of chance than a matter of certainty. The mathematician says such and such a thing is a matter of certainty; the engineer prefers to deal with matters of chance, and speculate and run a few risks. I am glad that these constants of Sir Hiram Maxim's have been determined, because I believe they may be of use in working out the result of longitudinal stability, and possibly they may be the means of Sir Hiram Maxim's original model being resuscitated. I believe as soon as mathematics can be brought into practical form, a lot of models of air-ships may be resuscitated, and put to simple tests for testing their stability, and it is that work which I hope to carry out. The next investigation is to try and reduce mathematics to a matter of simple calculation.

The Chairman: Professor Bryan said that mathematicians did not get any thanks for their work, but so far as we are concerned we will give him our thanks, and I am afraid it is all we have to give. I ask you to give your thanks to Professor Bryan—(applause)—and I will now ask Mr. Marriott to bring before us a paper on the balloon ascents made by the late Mr. James Glaisher for scientific purposes.

The Balloon Ascents made by James Glaisher, F.R.S., for Scientific Purposes, 1862-69.

By WILLIAM MARRIOTT, F.R.Met. Soc.

A memoir of the late Mr. James Glaisher, F.R.S., appeared in the April number of the *Aeronautical Journal*, so it seems hardly necessary for any further reference to be made to him. But since that time the instruments and apparatus used by him in his famous balloon ascents have, by the courtesy of his son, Dr. J. W. L. Glaisher, F.R.S., come into the possession of the Royal Meteorological Society.

Your Secretary, Mr. E. S. Bruce, was very desirous that these instruments should be exhibited at a meeting of the Aëronautical Society, and thought that it would be interesting to most of the members if a brief account of Mr. Glaisher's balloon ascents was given at the same time. I could not refuse this request, as Mr. Glaisher was for some time my chief at the Royal Observatory, Greenwich, although I think a more interesting account might have been prepared by someone else.

Mr. Glaisher was appointed Superintendent of the Magnetic and Meteorological Department of the Royal Observatory, Greenwich, on its foundation in 1840. He soon became interested in, and conversant with, all kinds of meteorological investigation, and through his instrumentality numerous meteorological stations were equipped in various parts of the country. He was practically the founder of the present Royal Meteorological Society, in 1850.

Mr. Glaisher became associated with the leading scientific men of the day, and as a member of the British Association he was able to take part in any discussion on meteorological matters. It is, however, only with the work which he carried out in connection with the investigation into the meteorological conditions of the upper air that we are to deal with to-night.

In the year 1852, Mr. Welsh of the Kew Observatory, made four ascents for scientific purposes in the great *Nassau* balloon, with Mr. Green as aeronaut. The heights reached were 19,500, 19,100, 12,640, and 22,930 ft. respectively. Owing to illness, Mr. Welsh was not able to make any more ascents.

At the meeting of the British Association at Leeds, in 1858, Col. Sykes urged the desirability of having further observations made in balloons on the decrease of temperature with altitude, and a Committee was appointed to consider how this could be carried out; but little or nothing was done. In 1861 the Committee was *re*-appointed, and then consisted of Col. Sykes, Chairman, Mr. Airy, Sir D. Brewster, Mr. Fairbairn, Admiral Fitzroy, Mr. Gassiot, Mr. J. Glaisher, Sir J. Herschel, Dr. Lee, Dr. Lloyd, Dr. Miller, Dr. Robinson, and Dr. Tyndall.

Arrangements were made with Mr. Green for the use of his balloon, and two young observers were instructed by Mr. Glaisher in the method of taking the observations. Difficulties, however, arose which frustrated the expectations of the Committee. The balloon was rent by the wind while it was being fitted for the first ascent, and subsequently it was found to be leaky and incapable of rising to any great altitude. In addition to this, the observers proved to be incompetent.

The Committee then approached Mr. H. Coxwell, the aeronaut, on the subject. After a

full discussion of the objects proposed, it became evident that if high ascents were to be made it would be necessary to construct a new balloon of greater capacity than previously made. The Committee were in despair at this, as the cost was beyond their means. Mr. Coxwell, however, generously undertook, at his own cost, to construct the required balloon, provided the Committee would assist. This they readily agreed to, and Mr. Glaisher then said that he would make the observations himself. And so it came about that the mammoth balloon was made, which was 80 ft. high, and had a diameter of 55 ft., and when fully inflated was capable of containing over 93,000 cubic ft. of gas.

The primary objects of the experiments as set forth by the Committee were the determination of the temperature of the air, and its hygrometrical states at different elevations, as high as possible.

The secondary objects were :—(1) To determine the temperature of the Dew-point by Daniell's and Regnault's hygrometers as well as by the dry and wet bulb thermometers, and to compare the results. (2) To compare the readings of an aneroid barometer with those of a mercurial barometer up to the height of 5 miles. (3) To determine the electrical state of the air. (4) The oxygenic condition of the air ; and (5) the time of vibration of a magnet : (6) To collect air at different elevations. (7) To note the height and kind of clouds, their density and thickness. (8) To determine the rate and direction of different currents in the atmosphere ; and (9) to make observations on sound.

This was a large and comprehensive programme, and Mr. Glaisher took steps forthwith to endeavour to carry it out. A special and elaborate set of high-class instruments was procured from the best makers, which were all carefully tested, and then Mr. Glaisher set to work to practice himself in manipulating and reading the instruments so that he might do so carefully and quickly when up in the balloon. He had a board, or table made, the extremities of which rested on the sides of the car, and on this he arranged the instruments in the manner shown on the screen.

The instruments were the following: — Mercurial and Aneroid Barometers; Dry and Wet-bulb Thermometers; Daniell's Dew-point Hygrometer; Regnault's Condensing Hygrometer; Maximum and Minimum Thermometers; A Magnet for horizontal vibration; hermetically sealed Glass Tubes, exhausted of air, and an electrometer. A camera was also taken up on one or two ascents.

This was the equipment for the high ascents. Mr. Glaisher was at one end of the car, facing the instruments, Mr. Coxwell being at the other end.

The instruments were fastened to the table with strings, which could be cut immediately, or they merely rested on stands which were screwed to the table. In descending, when approaching the earth, Mr. Glaisher rapidly removed the instruments and placed them in a basket between a number of soft cushions, so that they might not be broken by the shock on coming in contact with the earth.

The cost of each high ascent, which was made specially for the British Association, when Mr. Glaisher and Mr. Coxwell were the only occupants of the car, was about £50. As the grant of the British Association would only permit a few high ascents being made, Mr. Glaisher availed himself of the opportunity of accompanying Mr. Coxwell as often as possible in his balloon ascents from the Crystal Palace and elsewhere, which were made for exhibition purposes. On these occasions he paid his fee as an ordinary passenger in the car, but he had a small board fixed outside the car on which he placed the thermometers and hygrometers.

The accompanying table gives a list of the 28 balloon ascents made by Mr. Glaisher, 8 of which were in 1862, 8 in 1863, 8 in 1864, 3 in 1865, and 1 in 1866. The 4 from Wolverhampton and the 7 from Woolwich were undertaken wholly on behalf of the British Association Committee.

A full account of the various ascents, with the observations *in extenso*, will be found in the *Reports of the British Association* for the years 1862, 1863, 1864, 1865 and 1866.*

The highest ascent was that from Wolverhampton on September 5th, 1862, when the height of about 7 miles from the earth was reached. Mr. Glaisher was rendered insensible, while Mr. Coxwell's hands were frozen, and he was only able to open the valve by tugging away at the rope with his teeth.

I do not propose to discuss Mr. Glaisher's observations, but merely to give a brief summary of his own results. It must be borne in mind that self-recording instruments were not available as they are now, otherwise much more complete data would have been procured of the meteorological conditions of the upper air.

The following are some of the results of the experiments :—(1) The rate of the decline of

* By the courtesy of Messrs. Macmillan and Co., some pictorial diagrams showing the path of the balloon and the atmospheric conditions met with in several of the ascents were exhibited.

temperature with elevation near the earth was very different when the sky was clear from what was the case when it was cloudy, and the equality of temperature at sunset and increase with height after sunset were very remarkable facts which were not anticipated. (2) Even at the height of 5 miles cirrus clouds were seen high in the air, apparently as far above as they seem when viewed from the earth. (3) The results of the observations differed very much, and no doubt the atmospheric conditions depended not only on the time of day, but also on the season of the year, and were such that a vast number of ascents would be requisite to determine the true laws with anything approaching to certainty and completeness. It is also clear that England is a most unfit country for the pursuit of such investigations, as, from whatever place the balloon started, it was never safe to be more than an hour above the clouds for fear of reaching the sea. (4) It appeared from the observations that an aneroid barometer could be trusted to read as accurately as a mercurial barometer to the heights reached. (5) The time of vibration of a horizontal magnet was taken in very many of the ascents, and the results of ten different sets of observations proved, undoubtedly, that the time of vibration was longer than on the earth. (6) In almost all the ascents the balloon was under the influence of currents of air in different directions. The thickness of these currents was found to vary greatly. The direction of the wind on the earth was sometimes that of the whole mass of air up to 20,000 feet, whilst at other times the direction changed within 500 feet of the earth. Sometimes directly opposite currents were met with at different heights in the same ascent, and three or four streams of air were encountered moving in different directions. Ignoring the different currents of air which caused the balloon to change its direction, and at times to move in entirely opposite directions, and simply taking into account the places of ascent and descent, the distances so measured were always very much greater than the horizontal movement of the air as measured by anemometers. For example, on January 12th, 1862, the balloon left Woolwich at 2.8 p.m., and descended at Lakenheath, 70 miles distant from the place of ascent, at 4.19 p.m. At the Greenwich Observatory, by Robinson's anemometer, during this time the motion of the air was six miles only. (7) With regard to physiological observations, Mr. Glaisher found that the number of pulsations increased with elevation, as also the number of inspirations. The number of his pulsations was generally 76 per minute before starting; about 90 at 10,000 feet; 100 at 20,000 feet; and 110 at higher elevations. But a good deal depended on the temperament of the individual. This was also the case in respect to colour; at 10,000 feet the faces of some would be a glowing purple, whilst others would be scarcely affected. At four miles high Mr. Glaisher found the pulsation of his heart distinctly audible, and his breathing very much affected, so that panting was produced by the slightest exertion; at 29,000 feet he became insensible. (8) In reference to the propagation of sound, it was at all times found that sounds from the earth were more or less audible, according to the amount of moisture in the air. When in the clouds at four miles high, a railway train was heard; but when clouds were far below no sound ever reached the ear at this elevation. The discharge of a gun was heard at 10,000 feet, the bark of a dog was heard at the height of two miles, while the shouting of a multitude of people was not audible at heights exceeding 4,000 feet.

Owing to the great ascending power of the balloon at starting from the earth, the first few hundred feet were passed through too quickly to enable Mr. Glaisher to make satisfactory observations on the temperature and humidity of the air at the lower elevations He therefore, in 1869, readily took advantage of the offer of Mons. Giffard to permit him to make a number of observations in the car of the great captive balloon, then located at Ashburnham Park, Chelsea. This balloon was kept constantly inflated with 420,000 cubic feet of hydrogen gas, and it could ascend, on a calm day, to the height of 2,000 feet. Its rate of ascension could be regulated, and the balloon could be kept all but stationary at any point for any length of time.

The results of these observations supported those which were indicated by the free balloon ascents, viz., that the decrease of temperature with increase of elevation has a diurnal range, and differs at different hours of the day, the changes being the greatest at mid-day and the early part of the afternoon, decreasing to about sunset, when, with a clear sky, there is little or no change of temperature through several hundred feet from the earth, whilst with a cloudy sky it decreases from the mid-day hours at a less rapid rate to about sunset, when the decrease is nearly uniform at the rate of 1° in 200 feet.

No observations were made after sunset.

Before closing this paper I must refer to Mr. Glaisher's coolness in the balloon ascents. This is what Mr. Coxwell said of him after their first ascent together, when, in descending, they came

to the earth with a very considerable shock, which broke nearly all the instruments:—"I have frequently expressed my astonishment and admiration of the personal bearing of Mr. Glaisher in the balloon-car, when he undertook, without previous schooling and initiation, to go up to an altitude of five miles. I had never myself been to that elevation, but had been, when alone in Germany, as high as I could get with a balloon of 34,000 feet capacity; but this calm and self-possessed start on the part of Mr. Glaisher satisfied me as to his having the requisite qualifications for the duties upon which he had entered. I thought, too, that he even seemed to take a delight in the mode of travelling itself, and of the sight-seeing, scenery, and cloud-scapes which from time to time presented themselves, irrespective of the philosophical uses to which his notes and deductions were to be turned subsequently, when they were studied and gone into in his own systematic way. This to me was particularly gratifying, as I had read and heard of attempts by scientific men, years previously, who had trembled in the car, and could not do justice to their own powers and abilities for want of nerve."

In an ascent of the captive balloon at Chelsea on May 5th, 1869, a very strong wind was blowing. There were 29 persons in the car, including Mr. Glaisher. The wind whistled through the ropes, the balloon lay over, and the car oscillated violently. This is what Mons. W. de Fonvielle and Mons. G. Tissandier say:—"Mr. Glaisher appeared to pay no attention to the wind or to the oscillations of the balloon; his eyes remained fixed either upon his instruments or the dynamometer, which indicated a tension of three tons, therefore nothing to alarm us, since the cable could bear four times as much." I think the Aëronautical Society of Great Britain was fortunate in being under the guidance of such a practical and capable man as Mr. Glaisher in the early years of its existence; and I am glad that an opportunity has been afforded me of paying a humble tribute to his memory.

The Secretary: I have a tribute to the memory of Mr. Glaisher, who was practically

Glaisher's Balloon Ascents.

No. of Ascent	Date.	Ascent.		Descent.		Greatest Altitude	Temperature.		
		Time.	Place.	Time.	Place.		Start	Lowest	Finish
	1862.					Ft.	Deg.	Deg.	Deg.
1	July 17	9.43 a.m.	Wolverhampton ..	11.50 a.m.	Langham	26,177	59·0	16·0	—
2	,, 30	4.40 p.m.	Crystal Palace ..	6.30 p.m.	Singlewell ..	6,937	68·0	41·0	68·0
3	Aug. 18	1.2 p.m.	Wolverhampton ..	4.5 p.m.	Solihull .. .. 7 miles from B'm.	23,377	67·8	19·0	67·0
4	,, 20	6.26 p.m.	Crystal Palace ..	10.0 p.m.	Mill Hill	5,900	66·2	43·0	—
5	,, 21	4.30 a.m.	Mill Hill	7.10 a.m.	Biggleswade ..	14,355	60·8	19·3	61·8
6	Sept. 1	4.40 p.m.	Crystal Palace ..	6.15 p.m.	Woking	4,190	65·0	46·1	—
7	,, 5	1.3 p.m.	Wolverhampton ..	3.6 p.m.	Cold Weston ..	37,300	59·5	−5·0	57·2
8	,, 8	4.47 p.m	Crystal Palace ..	6.10 p.m.	Mucking Flats ..	5,428	67·2	49·3	—
	1863.								
9	Mar. 31	4.16 p.m.	Crystal Palace ..	6.30 p.m.	Barking Side ..	22,884	49·2	1·0	42·0
10	Apr. 18	1.16 p.m.	Crystal Palace ..	2.46 p.m.	Newhaven ..	24,163	61·5	11·5	57·0
11	June 26	1.3 p.m.	Wolverton ..	2.28 p.m.	Littleport	23,200	66·0	17·0	66·5
12	July 11	4.55 p.m.	Crystal Palace ..	8.50 p.m.	Goodwood Park ..	6,623	73·5	51·0	68·5
13	,, 21	4.52 p.m.	Crystal Palace ..	5.45 p.m.	Waltham Abbey ..	3,298	61·3	52·2	61·5
14	Aug. 31	6.12 p.m.	Newcastle-on-Tyne	7.5 p.m.	Pittington.. ..	8,033	64·0	33·5	53·5
15	Sept. 29	7.43 a.m.	Wolverhampton ..	10.30 a.m.	Temple Bruer ..	16,590	48·0	0·0	53·0
16	Oct. 9	4.29 p.m.	Crystal Palace ..	6.30 p.m.	Pirton Grange ..	7,310	54·5	26·5	—
	1864.								
17	Jan. 12	2.7 p.m.	Woolwich	4.19 p.m.	Nr. Brandon ..	11,897	42·0	11·0	41·8
18	Apr. 6	4.8 p.m.	Woolwich	5.17 p.m.	Nr. Sevenoaks ..	11,075	46·0	33·0	47·0
19	June 13	7.0 p.m.	Crystal Palace ..	8.15 p.m.	East Hornden ..	3,543	61·8	46·0	54·0
20	,, 20	6.17 p.m.	Derby	7.16 p.m.	Nr. Newark ..	4,280	65·0	49·2	64·0
21	,, 27	6.33 p.m.	Crystal Palace ..	9.21 p.m.	Romney Marsh ..	4,898	63·0	40·2	46·5
22	Aug. 29	4.6 p.m.	Crystal Palace ..	5.30 p.m.	Wybridge, Essex ..	14,581	72·5	28·5	69·0
23	Dec. 1	2.37 p.m.	Woolwich	4.35 p.m.	Nr. Rochester ..	5,431	48·0	29·1	45·2
24	,, 30	2.13 p.m.	Woolwich	3.10 p.m.	Stanford-le-Hope	3,735	42·5	27·2	38·2
	1865.								
25	Feb. 27	1.58 p.m.	Woolwich	4.1 p.m.	South Hanningfield	4,865	52·1	34·0 / 32·5	44·5
26	Oct. 2	6.20 p.m.	Woolwich	8.20 p.m.	Highmoor (Ox.) ..	1,949	56·0	—	56·0
27	Dec. 2	5.15 p.m.	Woolwich	7.0 p.m.	Ridgemouth ..	4,628	38·3	27·2	39·3
	1866.								
28	May 29	6.14 p.m.	Windsor	9.25 p.m.	Nr. Pulborough ..	6,325	58·0	29·5 / 33·5	50·2

the father of our Society—a simple laurel wreath to place at the feet of his bust, which the Royal Meteorological Society has so kindly lent us this evening, when we have to consider the work of this very great man, I think I may say one of the giants of science. I will now place this wreath at the feet of Mr. Glaisher.

The Chairman: Ladies and Gentlemen:—It is hardly necessary for me to add anything in the way of commendation to the work Mr. Glaisher did. His aëronautical work has made his name famous all over the world, in addition to his meteorological work, and in meteorological work we have an enormous amount to thank him for. We now at this day use his table for the reduction of wet and dry bulbs, so that the name of Glaisher is not likely to be forgotten for a long time; indeed, it is likely to remain as long as meteorology and aëronautics go on being studied, and I presume they will be studied so long as the world lasts. I think it is very fortunate that this subject has been presented to you to-night, because it is just possible that in the great development of scientific ballooning that takes place elsewhere the claims of this country in the matter may be overlooked. The balloon ascents of Welsh and Glaisher—and I am very glad Mr. Marriott mentioned Mr. Welsh's name, because Welsh's work was really of the highest class—the work of Welsh and Glaisher in scientific ballooning was pioneer work, and it is well that in these days the facts relating to them should be brought before the notice of the Society. I therefore have much pleasure in returning thanks on your behalf to Mr. Marriott for bringing the subject to our notice.

A letter received by Major B. Baden-Powell from M. de Fonville, late President of the French Aeronautical Society was read by the Honorary Secretary, expressing M. de Fonville's regret that he could not be present at the meeting, and the satisfaction of the French Aeronautical Society at the action of the Aeronautical Society of Great Britain "in paying due justice to the great work executed by Glaisher, which remains the basis of aeronautical science.

The Mechanical Imitation of Bird Flight.

By WILLIAM COCHRANE.

Mr. Chairman, Ladies and Gentlemen:—

In commencing the paper which I am about to read I would like, first of all, to make a few remarks as to what has been done towards solving the difficult, yet extremely fascinating problem of aërial flight.

In recent years much has been attempted, but, when all is weighed in the balance, I venture to think that comparatively nothing of any value has yet been accomplished, and the successful flying machine has yet to come. A debt of gratitude is due to M. Santos Dumont. M. Lebaudy, and other pioneers, for breaking down, as they have, prejudice against the subject; but, after all, do his or their machines embody any new principle? Only by careful attention to certain mechanical details and a combination of mechanism already in existence have they accomplished what they have.

It is unnecessary for me to go into details of the defects of navigable air-ships. Most students of aëronautics are, I think, agreed that the flying machine of the future will be one which is heavier than the atmosphere and presents the least surface of resistance when travelling horizontally.

I venture to go a step further and prophesy that some new principle must be introduced before any further important step is gained. The screw propeller must give place to a more efficient form of propulsion. My objections to the screw as a means of propulsion for aeronautical purposes are these: It is not a feathering propeller; it can only exert itself in one direction or plane; it cannot increase its propelling surface and maintain its efficiency as the velocity of the machine increases; it contributes nothing to increase the safety of the machine in the event of the motor failing; and, finally, owing to its excessive diameter it is very cumbersome. What is wanted, I think, is a feathering propeller which will present the least surface of resistance to the medium through which it travels, and will at the same time obtain the best grip. It should strike the mobile medium against which it works with a resilient blow alternately in opposite directions, thereby destroying all tendency towards cavitation and pounding, as at present existing in high speed screw propellers. Thus a maximum of resistance and efficiency should be obtained.

Further, an aëronautical propeller ought to have a combined lifting, propelling, and sustaining movement, the amount of propelling surface may be increased and the sustaining surface decreased as the velocity of the machine increases, or *vice versa*.

After long and close study as to the formation of the wings of certain birds, I have come to the conclusion that the primary feathers, that is,

those with the quill on one side of the film are lifting, propelling, or sustaining feathers according as the bird desires.

I have by the aid of a powerful telescope tried to find out if the feathers in the wings were controlled independently of the wing, but was unable to discover anything owing to the rapidity of the motion of the wings; however, I still think they are capable of independent action.

For instance, when the bird is getting off the ground, on the *downward* stroke of the wing, I think the edge of the film of No. 1 primary feather is pressed against the under-surface of the quill of No. 2 feather, the edge of the film of No. 2 is pressed against the surface of the quill of No. 3, and so on, thus forming a flat lifting surface on the downward stroke, like this

On the upward stroke of the wing the films by the action of the air on them are bent downward at an angle like this

thus forming a propeller. As the velocity of the bird increases, so it increases its propelling surface by spreading the feathers and permitting the films on some of the feathers to describe alternate oblique planes on the upward and downward strokes like this

DOWNWARD.

UPWARD.

This form of propeller I have successfully applied to a boat which gives better results than the ordinary screw propeller, and for aëronautical purposes a speed of anything up to 200 miles per hour could easily be attained with this form.

Now, referring briefly to what I may call gliding machines, I think they afford very dangerous amusement to those who use them. They might be all right if one could always fly downwards, but unfortunately the true theory of flight is *upwards*, against the law of gravitation, and that makes all the difference. I know that the advocates of this class of machine hold that by gliding one learns to keep one's equilibrium. That may be true when descending, but I think that the centre of pressure and the centre of gravity in a true flying machine would be entirely different. I think much would be accomplished if a machine were built on outlines to mechanically imitate bird flight. I have here a working model of a machine built on these lines, which I will proceed to briefly describe.

This is a flying machine heavier than air, composed of a combination of aëroplanes, oscillating wings, and horizontal tail-steering gear, it is built up from tubes which forms, as it were, a body from which the motor and car are suitably supported, about the middle, and fore and aft of the motor are respectively fixed the steering and raising gear. Wings are pivoted to the side members of the body, and are caused to rise and fall in unison in a manner resembling the wings of a bird by the movements of connecting rods actuated by the motor underneath.

The motor is of any suitable kind which will impart oscillating motion to the wings. The body tubes are preferably made hollow to act as a reservoir for the motor-driving fuel, such as petrol or the like. It will be seen that the wing frames are oscillated and act (by means hereinafter described) in such a way that their power is first exerted in not so much propelling the machine forward as in raising it from the ground. When the machine is high enough the full power is caused to propel it forward. To prevent air from escaping too quickly upon the down or "power" stroke of the wings a sail or other equivalent device is suspended from the outer sides of the wing frames, which sail is prevented from flapping outward by a cord or wire.

The wing frames are made as light as possible and are strengthened by binding rods or tubes, and upon each of these rods is pivoted a flap or vane. These flaps in one modification might be made of a sheet of aluminium or other material and made slightly concave, or they might consist of an outer edging or frame of a suitable metal or a stiff fabric, the frame being filled in with silk stretched loosely and varnished or otherwise so that on the up and down strokes of the wing, the silk would "belly" so as to obtain, as it were, a grip upon the air, and thus exert its utmost power.

From the foregoing description it will be understood that as the wing frames are oscillated, the flaps or vanes will form alternate oblique planes by impact with the air which results in a forward propelling movement being imparted

to the flying machine, but when starting from the ground, it is necessary that the action of the wings should not so much propel the machine forward as to cause it to rise from the ground. To obtain this end all vanes are prevented from completing their full stroke (*i.e.* are prevented from rising above the plane of the wing frames) by means of rods which are provided with a series of cranked or projecting portions alternating with straight parts which normally lie on certain vanes and prevent them completing their full strokes. The rods are pivoted in bearings mounted on the ends of wing frames and are capable of being partially rotated by suitable means from the car so that the cranked portions of the rods may be caused to lie flat upon certain vanes (preferably every alternate one) and when in this position, all the vanes will be prevented from completing their full stroke. When the machine has risen high enough the parts are turned up and the vanes will now be allowed to make a full or nearly full propelling stroke in proportion to how high the parts have been raised. The rods are fitted with tension springs to return them to their normal positions. To vary the extent of downward movement of the vanes one or more rods pivotally mounted on the ends of the wing frames may be moved up or down in a circular path by suitable means from the car. This will have the effect of varying the propelling power of the vanes. For example when starting the machine from the ground, *i.e.*, when the least propelling power is desired, the rods are allowed to drop to their full extent, thus allowing the vanes to fall as low as possible and form a fairly steep angle. When the machine has risen sufficiently high the rods are raised to reduce the downward strokes of the vanes; thus the vanes will be less oblique than before, so that on the up stroke of the wing frames they will present more propelling surface to the air.

To prevent the machine falling suddenly to the ground should the motor stop or break down, and to overcome the dead centres in the engine, strong leaf springs are disposed on the centre tube, and extend a short distance along the ends of the wing frames. By this means, the wing frames will be held in a horizontal position should the motor stop, and the wings, fore aeroplane and tail, will act as a parachute to prevent a sudden dropping of the machine.

The steering of the machine is effected by the steering tail. This consists of a central plane somewhat triangular in shape, and having side pieces, or fins, hinged to it. Tension springs normally act to pull the fins upward. Cords or wires adapted to be operated from the car, are attached to the fins, so that either of the fins may be pulled down to effect right or left hand steering, according to which fin is operated, or to enable both fins to be simultaneously pulled down to assist in raising or lowering the machine, according to whether the front aeroplane is in an up or down position.

The front aeroplane is for the purpose of assisting the machine to rise. It consists of a wood or metal frame, over which silk or canvas is stretched, and it is pivoted by its front end to the extreme forward end of the machine. The back end of the aeroplane is attached to cords, which can be operated from the car to vary its inclination. When the aeroplane is in an oblique position, and both the steering fins are down and the wings oscillated, the machine will be caused to rise. If, on the other hand the aeroplane is in a horizontal position and the steering fins are down, the machine will be lowered.

To diminish shock when the machine comes into contact with the ground, spring wheels may be fitted; these wheels would also prove useful when starting the machine from the ground, enabling it to receive a preliminary start, until the machine had acquired sufficient speed to raise itself from the ground.

The stroke of the wings may also be varied, and further to assist in steering or turning the machine rapidly round, the vanes of one wing might be checked in their strokes by the cranked rods, while the vanes on the other wing were fully propelling.

Finally, I beg to thank you, ladies and gentlemen, for your kind attention to my remarks.

Major Roberts: Ladies and Gentlemen—Before we leave, I am sure you would wish to give your cordial thanks to Dr. Napier Shaw, who is a very busy man in other walks, for coming here and presiding so well this evening.

The Secretary: Mr. Frost, a member of our Council, is unable to come and second the vote of thanks to-night, being laid up with influenza. I have just had a telegram put into my hands to that effect. Therefore, on his behalf, I, as Hon. Secretary, beg to second the vote of thanks to Dr. Napier Shaw for presiding.

The Chairman: I may accept the vote of thanks without words, and believe in so doing I shall consult not only my own gratification, but yours as well. I am much obliged to you for your patient attention to the long programme we have gone through.

The proceedings then terminated.

Applications for Patents.

(Made in October, November, and December.

The following list of Applications for Patents connected with Aëronautics has been specially Compiled for the AËRONAUTICAL JOURNAL by Messrs. BROMHEAD & Co., Patent Agents, 33 Cannon Street, London, E.C.

21209. October 2nd, 1903. JOHN FREDERICK DUKE. **Improvements in and relating to Propulsion of Air-vessels.**

21249. October 3rd, 1903. FRED HATTERSLEY PICKARD. **Improvements in or connected with Air-propellers.**

21331. October 5th, 1903. BADEN FLETCHER SMYTH BADEN-POWELL. **Improvements in Aerial Machines.**

21342. October 5th, 1903. THOMAS WILSON BISHOP. **An Air-ship without a Balloon.**

21415. October 6th, 1903. MARKUS MACK. **Half-cased stationary Paddle Wheel for air and water.**

21737. October 9th, 1903. BADEN FLETCHER SMYTH BADEN-POWELL. **Improvements in Aerial Machines.**

21769. October 9th, 1903. SIDNEY JOHN LAWRENCE. **An Improved Air-propeller.**

22040. October 13th, 1903. ALFRED FOSTER AND CHARLES EDWARD COX. **Improvements in Air-ships.**

23887. November 4th, 1903. HENRY SHEPLEY BOOTH. **Improvements in Aerial Machines.**

23920. November 4th, 1903. EDWARD GUESS. **Improvements in Propelling and Steering Air-ships.**

24490. November 11th, 1903. WALTER ELLIS. **Improvements in and relating to Machines for Aerial Navigation.**

24715. November 13th, 1903. JENÖ DE VARGYAS AND LAJOS LAKNER. **Improvements in and relating to Air-ship Propulsion and Apparatus therefor.**

24812. November 14th, 1903. SAMUEL DALZIEL. **Improvements in Propellers for Air-ships.**

25375. November 20th, 1903. REGINALD MANSFIELD BALSTON. **Improvements in or relating to Kites, Flying Machines, and the like.**

25378. November 20th, 1903. JOSEPH BARCLAY. **An improved means for Aerial Flight.**

25444. November 21st, 1903. ARTHUR HENRY PHILLIP BLOUNT. **Improvements in and in means for Propelling and Steering Aerial Machines.**

25692. November 24th, 1903. ALEKSANDER ZALUSKA. **Improvements in Flying Machines.**

25756. November 25th, 1903. SAMUEL GEORGE DALTON. **An Improved Flying Machine.**

25899. November 27th, 1903. WILLIAM LENNARD JACKSON. **Improvements in Aerial Machines or Air-ships.**

26070. November 28th, 1903. THEODORE HENRY STRINGER. **Improvements in Flying Machines.**

26821. December 8th, 1903. BADEN FLETCHER SMYTH BADEN-POWELL. **Improvements in Aerial Machines.**

26890. December 8th, 1903. OSCAR ROBERT RICHTER. **Improvements in Flying Machines and in Explosion Engines suitable for use therewith.**

27049. December 10th, 1903. ARTHUR HENRY PHILLIP BLOUNT. **Improvements in Aerial Balloons.**

37328. December 14th, 1903. HENRY GARWOOD WATEL. **Improvements in Air Propellers.**

27407. December 15th, 1903. JAMES TOWNSLEY PEARSON. **Revolution in Balloons, Air-ships, and Aerial Flight.**